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# PHYSICS.—*Advantages and disadvantages of various interpretations of the quantum theory.*[1]

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## STATEMENT OF THE PROBLEM

Joseph Henry's genius was attracted primarily to the great experimental problems of his age. Although he was a professor of natural philosophy, a discipline which a century ago combined the various branches of physical science, he is not known to have indulged in the kind of formal considerations to which the title of this evening's discourse alludes. Yet I am confident that my subject is not wholly inappropriate to the occasion of a lecture in honor of Joseph Henry. For his eminent biographer Charles Greeley Abbot describes him as "a man of varied culture, of large breadth and liberality of views, of generous impulses and of great gentleness and courtesy of manner." Hence, while the speculations on which I am about to embark can hardly aspire to honor his memory, we may take comfort in supposing that he would gracefully listen to them and accept them as a small token of respect.

Questions as to fundamental meanings have accompanied the development of the quantum theory from the beginning. They appeared in the controversy over the wave-particle dualism, in the problem of observability posed by Heisenberg's matrix mechanics, in the early pilot wave conjectures of De Broglie, in the complementary principle of Bohr. They have been revived by the recent publications of Bohm, De Broglie, Vigier, and Weizel. It is the attempts of these latter authors I should like to appraise in simple philosophic terms.

Contrary to widespread belief, the problem in question is not difficult to conceive or to explain. I shall endeavor to present it in its basic features, shunning the artifacts of mathematics, which often serve to becloud the scene. The details, it is true, can hardly be treated without analysis. But the details are not in doubt; the controversy concerns their interpretation. It is therefore proper to select for study the simplest possible instances of quantum mechanical reasoning and examine their bearing upon the issues of the present debate.

The first example I propose is the motion of a firefly in a dark summer night. To the eye, the motion of this insect is not continuous; what it presents is a succession of bright spots or streaks at different places in our field of view. The judgment that this phenomenon represents the uninterrupted passage of an object from one point of space to another is based, strictly speaking, on an interpolation between the bursts of luminosity that are actually perceived. Yet common sense, and indeed scientific description, regard themselves fully justified in performing that ideal supplementation of immediate perception which the interpretation of these sporadic darts as continuous motion demands. The chief reasons for this attitude are the following:

First, the hypothesis of continuous motion is testable through other experience. It is possible to watch the firefly in the daytime, when its progression from point to point becomes visible. This settles the issue in large part, although it may not convince the inveterate skeptic who feels that, when unilluminated, the firefly behaves like the angles to whom St. Thomas attributed the ability of emerging at separate points without having to traverse the intervening dis-

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tance. To answer the skeptic, we must demonstrate the simplicity and convenience of the continuity hypothesis. Thus we add to the fact of partial testability a second item of evidence of a more rational sort, namely, the simplicity of the geometric curve on which the luminous dots are situated. If the interpolated path were very irregular, showed unlikely curvatures and strange convolutions, doubts as to continuity might remain; the smoothness of the plotted trajectory goes a long way toward removing them.

The validity of every scientific theory, even the simplest, rests ultimately on two kinds of evidence: (1) empirical verifiability of some of its consequences, and (2) rational coherence, economy of thought, or simplicity conveyed by the ideas composing the theory.

Atomic entities, like electrons, present phenomena which, on the purely empirical side, are not unlike the sporadic emergences of a lightning bug at night. To be sure, the electron in an atom cannot be seen. Nevertheless if the results of experiments and observations using the refined techniques of modern physics can be trusted, an electron in what is called a Bohr orbit reveals its position as a random set of points located throughout a region of space in the neighborhood of the classical orbit. More precisely, if a series of position measurements were made while the electron is in the unvarying state known as the ground state of the hydrogen atom, the results would form a probability aggregate of known spatial distribution, the individual positions thus established will dot this region in a curious manner, offering no immediate suggestion as to continuity of motion.

Thus the question naturally arises: Can we regularize these emergences by the same principles we employed in concluding that the path of the lightning bug was continuous? Or do we confront here a situation calling for entirely different treatment?

Unfortunately, the road leading to empirical verification of the continuity hypothesis is blocked, not merely by incidental obstacles arising from imperfections of measurement or observation, but also by infelicities of a fundamental kind. The electron is intrinsically too small to be seen; the act of vision, even if it were possible, requires a time too long for a clear ascertainment of instantaneous positions; last but not least important is the fact that elementary particles are promiscuous entities with a perversity which prevents us from ever being sure that we see the same individual in different observations. If these difficulties seem inessential, if hope still remains that they may be overcome in the future, then we need to remember that their denial contradicts the basic tenets of the quantum theory, the only theory capable of explaining what can in fact be observed about electrons. The conclusion is inescapable: there is no daytime in which the electron's path could be watched.

Let us therefore examine the continuity interpretation from the point of view of simplicity or economy of thought. Here we encounter another failure. A curve drawn through the measured points becomes complicated and aimless, wandering in erratic fashion, with no preference for connecting neighbors, a curve intertwining and crossing itself in obvious labour to accommodate the positions of the particle. Certainly nothing is gained in ease of conception, in plausibility, or in power of prediction by this familiar artifact.

Thus it is seen that the physical microcosm, the atomic realm, confronts the physicist with a novel kind of problem in interpretation, with a challenge to simplify or rationalize perhaps in ways to which he is not accustomed. And nature is not generous in providing hints for the solution of this methodological puzzle; the difficulties of direct verification we have already noted are so great that theories cannot be readily exposed to test. The sphinx is noncommittal. The physicist has an embarrassing amount of freedom in making his interpretations.

And how happily would he welcome the logical experts on "theory construction," the men who have put scientific procedure in pigeon holes, to whom the facts suggest inductively an hypothesis with computable probability. Here is a place where the principles of theory construction could be tried in vitro, with benefit for science itself. But nothing seems to be happening to relieve the suspicion that there are no recipes for

constructing successful theories, that the creative act in factual discovery as well as in theoretical interpretation refuses to be codified. Let us return, then, to the physics of the situation and examine the proposals in terms of which the antics of the electronic lightning bug have thus far been rationalized.

### THE MECHANISTIC THESIS

There are three distinguishable views with possible gradations between them. The first of them, which I have somewhat bluntly called mechanistic, is a continuation of time-honored procedures in classical physics or, in the eyes of its opponents, an obsolete hangover from an unenlightened past. It persists in portraying phenomena continuously in time and space despite the difficulties we have noted; it goes on using visual models where vision palpably fails. It reaffirms the convictions of a number of famous nineteenth-century scientists (Maxwell, Kirchhoff) who saw the aim of all science in the discovery of models which allow an understanding of phenomena by their interaction in time and space. De Broglie, one of the foremost advocates of this school, identifies pictorability in time and space with "clartó Cartésien."

The word mechanistic is literally applicable only to the simplest variety of space-time interpretations; others are more refined and complex, introduce nonmechanical agencies like fields, both three- and many-dimensional, but continue to avow the real existence of specific world lines, of detailed trajectories in a continuous space-time manifold. These latter formulations, which include the theories of De Broglie and Bohm, might be called quasimechanistic, in a deep philosophic sense however they are related to the others and I shall not hesitate to deal with them as special versions of the mechanistic thesis. Our attention must thus be directed, under this heading, to two related attempts at explanation, one simple and the other more refined.

The simple one sees the cause for the firefly behavior of electrons in the havoc wrought by the measuring process. It holds that the electron has a perfectly determinate position at all times, but this position is disturbed by the photon which, on being reflected, carries to the eye or to the measuring device the information where the electron was. The photon imparts to the electron a recoil which in consequence makes the position of the latter object uncertain. It will be recalled that all early explanations of the uncertainty principle invoked mechanical processes of this kind: transfer of momentum, or energy, scattering, lack of temporal precision of the measuring act with consequent uncertainty in the position of the observed object. Furthermore, if the particle aspect did not yield a convincing demonstration of uncertainty, there was always the wave nature of electrons to be drawn upon for further evidence.

The inherent plausibility of this reasoning is strengthened by the circumstance that it shows why atomic particles are erratic and the objects of our daily lives are not. A single photon represents a negligible disturbance when it impinges on bodies of ordinary size, but is a very energetic and disarranging missile when fired upon the miniscule electron. And the quantum theory shows that its energy cannot be made arbitrarily small, its amount being fixed at $h\nu$.

Why, then, does this simple explanation fail to command universal acceptance? Its shortcomings are fairly impressive: In the first place there *are* unquantized missiles such as other particles, for which the last preceding argument does not hold. Secondly, it is hard to see why the measuring disturbance should destroy the state *before* the measurement, why it should not convey information as to what the electron's position was prior to the act of collision. An operation can effect a successful diagnosis of a disease even if its kills the patient! Finally, to extend the criticism, it is possible to show that the uncertainty principle, which we may look upon as a quantitative expression of the erratic behaviour in question, is a consequence of the basic laws of quantum mechanics and makes no direct reference to the destructive effects of measurements.

None of these objections, however, is entirely conclusive; they are merely irritating and can be removed by clever reasoning, chiefly by a skillful use of the so-called wave-particle dualism. But the major blow to the simple mechanistic view comes from

the realization that, even if it is adopted, it provides no opportunity for *calculating* or *predicting* the mysterious disturbances that confuse the otherwise clean trajectories. It asserts their presence and resigns. It forms an idle embellishment of the facts and yields perhaps esthetic, not scientific or philosophic, satisfaction.

The refined version of the mechanistic thesis promulgated by De Broglie, Bohm, and Vigier is largely immune to such elementary criticisms. It is far more explicit and does an analytically competent job of interpreting the fundamental equations of quantum mechanics. It seizes upon a well-known connection between the Hamilton-Jacobi equation of classical physics and the Schroedinger equation, splits the latter into a pair of real equations, one of which can be used to define a path for the electron. This path is disturbed, not by interfering measurements, but by a nonclassical field arising from the presence of the electron itself. When a measurement is actually performed, this somewhat mysterious field, in conjunction with the measuring instrument, brings about the emergence of the particle at the place of registration.

Admittedly such reasoning is complicated and, because of its appeal to an unorthodox and special kind of field, perhaps in need of treatment by Occam's razor. But the formal structure of the theory is without flaws, and it is developing, chiefly through the work of Bohm, into an amazingly consistent formalism. De Broglie, it is true, takes exception to the lastest forms of it, but on grounds of extrascientific convictions and of the lack of plausibility of the quantum field. Briefly, his objections are these:

(*a*) According to Bohm's interpretation an electron in an *S*-state does not move, thus contradicting a notion familiar in physics since the days of the Bohr theory.

(*b*) The state function ($\psi$) can not represent physical reality because it is complex, and it extends in configuration space of many dimensions, not in ordinary space. (This argument can be met by supposing that the forces conveyed by the nonclassical field are complicated many-body forces.)

(*c*) Finally, De Broglie points out, the physical act called measurement is turned into a mystery, for it involves a sudden, infinitely rapid collapse of the $\psi$-field, which before the measurement filled all space, upon the immediate locus of the electron.

This is not the place to examine the soundness of the foregoing strictures; some of them have plagued quantum mechanics from its beginning and apply to other interpretations as well. The last point, which adverts to the sudden disappearance of the $\psi$-field upon measurement, is often made and presents in my opinion an insurmountable difficulty to every mechanistic interpretation of quantum mechanics. But more of this later. Let it be noted for the present that the controversy involves no questions of empirical fact and that the view here outlined is perfectly tenable in the face of what is now established scientific knowledge.

Before turning to another doctrine I should like to say why I did not follow the custom of calling the interpretation here under review a *causal* one. That adjective is correct but not discriminating, for there is another type of description, outlined hereinafter, which in a certain sense is causal too. What characterizes Bohm's ideas is a narrowly mechanistic form of causality, not causality in its widest scope.

## THE FORMALISTIC THESIS

Many a modern scientist will question the wisdom of indulging in considerations as speculative as the preceding, may even doubt that they have meaning. He will ask: do we not have a formal theory satisfactory for making valid predictions about things that matter? Why bother about interpretations beyond necessity? This positivistic attitude takes what is good and useful in modern theory, systematizes it as well as possible, and does not feel the pangs of conscience that afflict the tenderhearted metaphysician. The view sympathetic to it may be sketched as follows:

It takes the vagaries of the electron as facts. If pressed, it regards them as symptoms of disturbances by measuring devices but grants that every attempt to predict them or to understand them in detail is useless and in need of discouragement. *Particles have positions in space and time under all circumstances*, but atomic nature is

so constituted that we can often not know them. Because of this, a single measurement of position—or in general of any observable attribute—can not function as the basis of a precise and valid prediction.

To make some sort of prediction possible the physicist introduces his $\psi$-functions, which are essentially measures of information. Being incomplete as carriers of information, these functions permit only statistical predictions concerning aggregates of future events. Individual events, though always embedded in continuous temporal and spatial sequences, thus lose their effectiveness as causal agents in the physical world. To restore causality in a statistical sense, another description is required, a description in terms of $\psi$-functions, which are often waves. The universe of happenings is thus divided into two separable strands of development, one consisting of events in space-time with real but unknowable connections between them and devoid of causal nexus, the other a ghostlike space-time manifold of causally evolving states whose relation to observable events is but statistical.

The square of $\psi$ in this interpretation, as in all others, represents a probability; but here a probability of a rather special kind. As is well known, probabilities are sometimes regarded as subjective measures of knowledge or belief, sometimes as objective frequencies (or limits of frequencies). Subjective probabilities change discontinuously with evidence, the others do not. Thus, for example, before a die is thrown, the subjective probability of the appearance of a five is $\frac{1}{6}$. After a throw it is either 0 or 1. The objective probability is $\frac{1}{6}$ at all times, for the frequency always refers to a large aggregate of throws and is unaltered by a single event. The formalistic view, insofar as it has become explicit on this issue, adopts the subjective meaning of probability. It assumes, for instance, that the $\psi$-function suddenly collapses from the field-like distribution throughout space to a small, pointlike residue at the instant of a measurement.

Bohr's authority stands impressively behind this doctrine. He speaks of it as the principle of complementarity and regards it as the final form of physical analysis. The two modes of describing our experience, irreconcilable in man's mind, are the best we can achieve; the dualism they imply is here to stay. It stands as a memento to the fundamental truth that, in exploring nature, we become disturbing (or, if you will, creative) agents and thereby alter what would otherwise have been the case.

This view, appealing because of its candor and its seeming modesty, is espoused in its essence by the majority of physicists. To acknowledge the dualism has many soothing advantages, as every other form of dualism does. It relieves its advocates of the need to bridge a chasm in understanding by declaring that chasm to be unbridgeable and perennial; it legislates a difficulty into a norm. It is little wonder, therefore, that philosophers at times feel ill at ease when studying this solution of a dilemma, a solution which pays its respects to both horns. But the reward for this accomplishment is quite considerable: it gives the physicist a powerful philosophic tool. Clearly, if the most fundamental of all sciences has to accept complementarity, is it not natural that bifurcation should also pervade the lesser realms? Are not the mind-body problem, the conflict between values and fact, between freedom and necessity, mere manifestations of complementarity?

I fear that my own lack of sympathy with these extrapolations of the formalistic thesis has been ill concealed. Bohr's own cautious formulation does not suffer from such indiscretions. Yet it does commit physics to a dualism which is neither simple nor illuminating.

### A THIRD INTERPRETATION

It is possible to avoid the dualism by an interpretation which is philosophically more radical and more profound, a view that asks for a surrender of certain familiar habits of thought and a few cherished conceptions. To many, this price seems too high. I shall try to make this thesis as reasonable as possible, for it is the one which in view of all present evidence I find most congenial.

Why not simply deny that the electron has a position at all times? The real firefly partakes of "simple location," to use Whitehead's phrase, for the reasons we have mentioned: its path can be directly inspected

and the use of continuous interpolation between uninspected points leads to a simple and reasonable theoretical account. Neither is true for the electronic lightning bug! Of course one feels, initially, that somehow the electron must have a position, that position is an essential property of real material things. But this is clearly an example of accepting what Whitehead calls the fallacy of simple location. As we learn more and more about the world, we are asked to sacrifice in increasing measure the facile and picturesque presumptions of what we call so ineptly "common sense". It was common sense that argued that all physical entities, to be real, must occupy space; must have color even if they are smaller than a wavelength of visible light; must have definite shapes even if invisible; it was common sense that said the universe must be Euclidean, simultaneity must be absolute, and there must be an ether. The present situation, it seems, demands the courage and the modesty to disavow common sense; courage in the sense of D'Alembert's admonition, "Allez en avant, la foi vous viendra"; and humility to grant that knowledge in one domain does not render us wise enough to foretell another.

In the spirit of these injunctions we ought perhaps to admit that position—and with it many other observables—are undergoing the fate that befell the idea of color: it is not generally applicable to things that are too small or too elusive to be seen. Nor is it proper to ask whether such objects are particles or waves; the very denial of the unrestricted meaningfulness of the concepts position, size, etc., prevents it from being answered. Note, however, that this acknowledgement does not destroy our right to affirm the electron's presence as an objective component of reality. For it merely substitutes certain abstract qualities for those we deemed obvious and immediate; it substitutes mathematical models for mechanical ones. Logically, there is no reason why the character of an entity should be described by a visual image rather than a Hamiltonian.

The view in question is the culmination of a philosophic development of long standing. Galileo introduced the distinction between primary and secondary qualities, Locke and Descartes employed it significantly in their own philosophies. Primary qualities are those which are resident within their object; they are inalienable from it and make up its essence. Secondary qualities arise in the act of perception, are subjective in the simple sense of that word and are therefore less certain. To many of us, size, mass, atomic structure are primary qualities of a material body, whereas heaviness, color, temperature are secondary. But the very recital of such specific examples already tends to be embarrassing, and I for one would not care to defend the assertion that mass is primary and temperature secondary. Yet in early Greek philosophy, Anaxagoras thought it perfectly plausible to assign to his "elements" (homeomerics) the intrinsic property of taste.

Even this superficial account suggests what has in fact taken place throughout the history of natural philosophy. Primary qualities, first posited and affirmed with innocence and scientific blissfulness, engaged in a continual retreat before the onslaught of science. One after another of them was converted into a secondary quality, until today we are wondering whether perhaps the distinction is illusory, whether perhaps all qualities are secondary.

To sharpen this issue, I propose a shift of attention. The distinction between primary and secondary qualities is indeed of lesser interest today and may be regarded as settled, as Jeans believes. But though it be dead, its ghost is still very much alive and amongst us. The contrast, or at any rate the difference, is now between what I have called elsewhere *possessed* and *latent* observables. Possessed are those, like mass and charge of an electron, whose values are "intrinsic," do not vary except in a continuous manner, as for example the mass does with changing velocity. The others are quantized, have eigenvalues, are subject to the uncertainty principle, manifest themselves as clearly present only upon measurement. I believe they are "not always there," that they take on *values* when an act of measurement, a perception, forces them out of indiscriminacy or latency. If this notion seems grotesque, let it be remembered that

other sciences, indeed common sense, employ it widely. Happiness, equanimity are observable qualities of man, but they are *latent* qualities which need not be present at all times; they, too, can spring into being or be destroyed by an act of inquiry, a psychological measurement. The third interpretation regards the position of the electronic lightning bug as a latent observable.

It is less committal than the others. For clearly, if the electron did have a determinate position at all times and we could not possibly know it, this view would still stand aright. Likewise, it is compatible with, though again less committal than, the appeal to measurement as bringing about this latency. Perhaps it is an instrumental disturbance that does it, perhaps—and I should favor this conjecture—there is an irreducible haziness in the very essence of perceived phenomena of which Planck's constant $h$ is the quantitative expression. It may be that this latency affects even the identity of an electron, that the electron is not the same entity with equal intrinsic observables at different times. The suggestiveness of the hypothesis is evident, and with it the danger of mysticism. When the view is shorn of its extraneous implications, it avers that the electron *is* where it is measured, that it may be nowhere when it is not measured, that a measurement, properly contrived, may cause it to appear somewhere. The advocate of this view is not entitled to speculate about real trajectories, to follow his mechanistic propensity of picturing the motion of an atomic particle accurately in space and time (except as an approximation). We thereby cut off one horn of the complementarity dilemma and take as the only valid description of reality the $\psi$-function formulation. Before seeing what that entails, let me insert another word about the difference between possessed and latent observables, a speculative word.

I believe that this contrast, like that between primary and secondary qualities, will ultimately be resolved in favor of the latent observables, that is, the representation of physical observables in terms of operators rather than $c$-numbers is probably fundamental, and we shall perhaps find suitable operators for charges and masses as we have for positions, momenta, energies, spins, and all the rest. The fact that under certain conditions quantization, uncertainty and latency seem to be absent, as in the large-scale world, is guaranteed by Bohr's correspondence principle, which is not a special postulate but can be derived from the axioms of quantum mechanics.

Now, in what sense can the shadowy $\psi$-functions of the Schrödinger equation be real? Let us translate the question into the familiar terms of the lightning bug phenomenon. The $\psi$-function, when squared, represents the probability that a speck of luminosity will appear in a specified volume under scrutiny, or still less technically, the number of times I see a speck divided by the number of times I have looked. There is nothing vague or tentative about such probabilities; they are numbers obtainable by observations just like those which describe all other physical fields. The only difference is that a probability number requires numerous observations in order to be established, whereas an electric field strength can in principle be determined by a single observation. In practice, however, the physicist is able to perform his set of observations in a single act because he has available a large number of similar atomic systems. For example, a single illumination of hydrogen atoms by an X-ray beam produces a pattern on a photographic plate from which the probabilities of position for an electron can be inferred. Hence even the one methodological distinction between a probability field and other fields is largely academic.

Yet physicists, mindful of earlier theories which used probabilities only *à faute de mieux*, have come to associate with them a flavor of ignorance, a mental quality; they often regard them as subjective appraisals of a situation not completely understood, or as intrusions of metaphysics into the objective scheme of things. There are many signs on the horizons of modern science which belie this view, interesting new developments in mathematics, statistical mechanics, and information theory that lie beyond the scope of this account. Hence one may well regard the denial of real status and fundamental importance to probabili-

ties, which is so characteristic of classical physics, as an outmoded attitude. This leads me to suggest that we grant consciously to probability the function which in fact it already assumes: to serve as the fundamental determinant of experiences in a real world.

After all there is nothing illogical in the seemingly grotesque conception of probabilities flying about in space! Their relation to observational experience is certainly no more remote than the connection between a light wave and its visual manifestation, or indeed between the observed emission of a beta ray and a neutrino field. Nor does it put any strain upon common sense in the world at large, for the correspondence principle converts all probabilities referring to ordinary objects into $\delta$-functions (i.e. pointlike concentrations at the place where the object is conceived to be), and there is no difference between probabilities flying through space in the form of $\delta$-functions and classical objects!

This third interpretation is simple as a philosophic doctrine, monistic by virtue of its rejection of detailed particle trajectories, objective because it takes its probabilities as measurable fields and not as indexes of knowledge or belief; unfortunately, however, it demands a maximum departure from familiar lines of thought. I have chosen not to name this view because it is difficult to label in a simple way. V. F. Lenzen (*Causality in natural science*, 1954) calls it the objective view because it ascribes objective reality to probabilities. This terminology seems to me very appropriate.

On what grounds are we to judge these three interpretations? Is the final verdict as to their validity a matter of personal taste? The state of affairs here is quite different from what it ordinarily is in science; no crucial experiment being available for discrimination. In such cases recourse can and must be taken to the principles of scientific methodology, for in the last analysis these provide the criteria which every good scientific theory must satisfy. I shall therefore give a brief review of these principles.

## METHODOLOGY OF SCIENCE

Science serves to make reasonable or understandable as large as possible a portion of our experience. Certain parts of experience, like fleeting sensations, unrelated perceptions and observations, are in themselves devoid of rational order. Science strives to make them coherent, not so much in their direct setting, but by carefully associating with them specific ideal structures sometimes called concepts or constructs, and by endeavoring to reproduce among these meaningfully related structures the perceptory sequence of immediate facts. Permit me, to avoid circumlocutions, to state the essence of scientific method in somewhat arbitrary but pictorial terms (cf. *The nature of physical reality*, 1950).

The figure represents what one might call a section of our (cognitive) experience. Its limit is the P-plane (P for "perception" or "protocol" [public record]), the locus of immediate perceptions, observations, data or anything else we deem incontrovertible. It is in a sense a boundary of our experience, because we do not go beyond it to anything more ultimate in science as such. To the left of the P-plane extends a vast domain pervaded, as it were, by rational texture. It is the field of concepts or constructs (C-field), populated by originally ideal entities which habit, plausibility considerations, or outright postulation has associated with the data on the P-plane. The linkages between the P-elements and the C-elements will be called rules of correspondence.

Under certain conditions, to be outlined presently, the constructs take on scientific validity, assume an approved status as real entities in the world or, as I shall briefly say, become verifacts. In popular, ontological language, these verifacts "exist."

It is clearly of prime importance to know the criteria under which the transformation from the tentative character of a construct to the approved state of a verifact takes place. These criteria may be found, not in speculative conjectures about first principles or rules of thought, but through a study of the actual procedures in historical science. Such a study, it seems to me, yields two classes of verifying conditions.

In the first place, the constructs employed in scientific explanation must satisfy certain vague formal requirements which often go under the names of coherence, neatness, or economy of hypotheses. Secondly, they must

"agree" with the facts of the P-plane. Let us call these two requirements the *metaphysical* and the *empirical* criteria. Each is, of course, in need of a more meticulous analysis than this brief survey can undertake; their general features, however, can be sketched.

The meaning of the second, the empirical set of requirements, is illustrated by the line E, which starts on the P-plane, moves via rules of correspondence to the C-field and finally returns to P. It represents a typical *circuit* of empirical verification. Some observations (like Newton's falling apple) suggest constructs (mass, acceleration, force of gravitation) which, when combined in accordance with theoretical rules applying to these constructs, allow a return to the P-plane at some other place (motion of the moon). Thus, on the basis of some initial facts, a prediction of other facts has been made. Circuits of this kind are extremely numerous, and each can be traversed in both directions. When a given set of constructs, a theory, has been crossed by a sufficient number of circuits like E, it is said to be empirically valid.

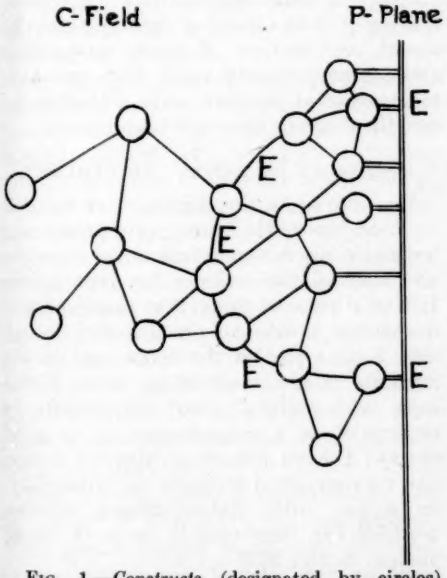


FIG. 1.—*Constructs* (designated by circles) are connected by formal relations (light lines) to one another; some are linked by *rules of correspondence*, which usually are operational definitions, to the plane of perception (P-plane). Metaphysical requirements regulate the C-field; verified sets of connected constructs, i.e. accepted theories, can be traversed by circuits of empirical confirmation, one of which is drawn as E.

The metaphysical requirements are of another sort, for they do not relate to specific matters on the P-plane. Rather, they constitute ideal devices by means of which the a priori fitness of the constructs is appraised. No attempt will here be made to present them in their fullness. Let us rather discuss the three which are of greatest relevance to the interpretations of the quantum theory.

In the first instance, the constructs of a theory must be so chosen and connected as to permit continuous and uniquely determinable sequences of states. This is often called the postulate of *causality*; it is also regarded by many as differentiating between the mechanistic thesis which is said to obey it, and the other two interpretations which do not. The principle of causality in what seems to me to be its simplest and clearest form requires only this: that physical systems be described in terms of states which are self-unfolding in a determinate manner; that the state of a system at time $t$ be sufficient for a prediction of the state (i.e., the values of the same crucial variables) at any other time $t'$. The principle does not spell out what these states must be, leaving mechanics free to operate with positions and momenta of particles, electrodynamics to use field variables, hydrodynamics to use pressures and velocities at points. Nor does it discriminate against the use of $\psi$-functions in quantum mechanics. And these $\psi$-functions are elements of a causal description whether $\psi$ refers to an ensemble of trajectories as in Bohm's interpretation; or whether it is the probability amplitude of a statistical ensemble. Only if the special mechanistic version of causality, the version which requires that prediction be based on *single* observations or *individual* events, is given unique and preeminent importance, does the third interpetation become noncausal. However, this narrow insistence does violence to a wider methodology of science and is difficult to justify.

Next among the three metaphysical requirements I have chosen to offer for consideration is *extensibility*. A theory must be

extensible to a large domain of facts. Science prefers that one among rival theories which is applicable to the greatest number of phenomena. From this point of view Newton's theory of dynamics was preferable to Aristotle's, Maxwell's equations are preferable to theories of Faraday and Ampère, Einstein's general theory of gravitation is preferable to Newton's, the quantum theory to classical dynamics. The principle of extensibility (or extensiveness) itself is vague in logical contour; one cannot say in any given instance whether a theory is sufficiently extensive or not. Its power arises, as with all metaphysical principles, from the fact that the scientist is apparently always able to form an intuitive judgment with regard to sufficiency. Even more effectively can he use the principle in discriminating between competing theories.

Finally, there is the requirement of *simplicity*. Again, I do not feel the need for defining the exact meaning of simplicity, nor do other scientists who use this idea in their appraisal of theories. In practice, and certainly for our present purposes, its intention is clear, I am sure. Galileo's description of free fall was simpler than, say, Tartaglia's; Newton's theory of motion simpler than Aristotle's; Copernicus' astronomy simpler than Ptolemy's; the electromagnetic theory of light simpler than the late ether theories; the *S*-matrix approach in nuclear theory is simpler than the use of different nuclear potentials on different occasions, and so on. These requirements are felt in all disciplines given to careful thought: even philosophers prefer monism to pluralism because of its better accord with the requirements of extensibility and simplicity.

Having now come to the end of our sketch of the methodology of science, we are perhaps in better position to judge the advantages and disadvantages of the interpretations of quantum mechanics described hereinbefore. It should be acknowledged, however, that the situation under study differs from those normally met in science, and presents unusual difficulties, because of the paucity of decisive data on the P-plane. For curiously, the *known* facts are explained by all three interpretations, and unknown facts crucial to one thesis and not to the others are extremely difficult to obtain. It is evident, therefore, that we are forced to place an abnormal reliance on the metaphysical principles. By and large, the circuits of empirical verification start at the same points on the P-plane and end at the same points in the three different interpretations. And where the mechanistic thesis does suggest possible discriminating observations, experience is noncommittal.

As a case in point I refer to a paper by Weizel (Zeitschr. für Phys. **134:** 264, 1953). This author takes the mechanistic thesis seriously and considers what, in mechanical terms, Bohm's quantum mechanical field might be. He asks: what kind of physical entity, thus far undiscovered, could possibly interact with the invisible firefly in a manner producing its erratic appearances? It must be able to act on the firefly without suffering a reaction itself, and this is a difficult assignment. But Weizel does find a suitable mechanism which he calls a "zeron"; he visualizes it as a sort of jellyfish moving with the speed of light, yet able to absorb an electron of given momentum and to spew it forth again with the same momentum at another place. Needless to say, these zerons have not been found.

## ASSESSMENT OF MERITS AND DEMERITS

We now bring the principles of method to bear upon the three interpretations, hoping to reach some verdict. Let us make sure that all the evidence has been heard. I have reason to think that many of you doubt this point and are disposed to say that I have packed the court against the interests of the mechanistic view. For I have said nothing about *pictorability* of constructs as a requirement for a good theory; I have placed an abstract notion like entropy, field strength or probability, on a par with Rube-Goldberg devices. Nothing has been said in favor of visual models. Is this fair?

To be sure, most of us find pictorable models like billiard balls or waves highly desirable and convenient; indeed we often use them in our reasoning when we know we should not. They are suggestive, conducive to clarity of thought. The reason is

doubtless psychological: our sensory experience is strongly colored by our visual sense; people learn most easily by seeing. But it is also true that science has carried us very far beyond the range of vision, and to assume that pictures are useful where vision fails is wholly without logical cogency. On the other hand, physics uses nonpictorial elements with great success, as in electrodynamics. This sometimes fails to be recognized because physicists gain familiarity with E and H through use and then mistake what seems familiar for what is pictorable.

My own uneasiness about including pictorability in the list of metaphysical requirements arises from the intolerable way in which it contradicts or curtails both extensiveness and simplicity. If physics were to insist on it, its methods would not embrace the present procedures of Gestaltism and behaviorism in psychology, of social theories and economics. For such constructs as Gestalt, drive, habit, supply and demand have very little in the way of mechanistic pictorialness. This is my primary reason for omitting the (pseudo-) postulate in question.

A while ago I spoke of rendering a verdict; yet this is hardly what the occasion demands. We have seen that the scientific evidence is not complete and that only half the resources of scientific methodology, namely the metaphysical ones, can be drawn upon. Let us therefore temper our judgment with modesty and concede that part of it depends on taste. We are somewhat in the position of a literary critic evaluating three poems and cannot expect finality or general acceptance of our conclusions. Or, with greater optimism, we may consider ourselves in the position of a teacher who grades three themes, themes which he does not fully understand.

Here are the marks I would assign. On the score of causality, the mechanistic thesis gets a perfect mark; but the third interpretation ranks equally, for we have agreed not to discriminate unfairly between mechanical and statistical causation. The formalistic view renounces causality in its space-time description but retains it in the complementary $\psi$-field. Hence it would seem to merit half-credit on this score, i.e., five out of ten.

Extensibility seems equally great for the second and third interpretations. Bohr's complementarity finds application in many realms of thought; it has been acclaimed even by theologians as casting light on their problems (e.g., freedom of the will). The last view, which regards probabilities as irreducible and admits latent qualities, is very close to the thinking of psychologists, social scientists and modern statisticians. It too is compatible with the possibility of freedom though it provides no solution for it. The mechanistic thesis, on the other hand, is of use primarily in the physical sciences. Furthermore, it makes a paradox of human freedom. Hence a fair rating on the score of extensibility would seem to be: Mechanistic view 2, formalistic view 8, third view 8, the perfect mark being 10.

Finally we come to simplicity. Here it appears that the formalistic thesis scores very low, since it resigns itself to a dualistic explanation of nature. I would rate it 20 percent. The mechanistic thesis does not do much better because it encumbers the conceptual scene with ideas not needed in the third interpretation, which is the simplest of the three. To these two views I would assign, respectively, the marks 50 percent and 80 percent. The summary is given in table 1.

TABLE 1.—SCORE BASED ON METHODOLOGICAL REQUIREMENTS OF THEORIES

| Interpretation | Principle | | | |
|---|---|---|---|---|
| | Causality | Extensibility | Simplicity | Total Score |
| Mechanistic | 10 | 2 | 5 | 17 |
| Formalistic | 5 | 8 | 2 | 15 |
| Third | 10 | 8 | 8 | 26 |

The outcome of this test will be radically changed in favor of the mechanistic thesis if one or more of several possible contingencies occur. If Vigier, De Broglie, or Bohm succeed in their present endeavor to derive the equations for the quantum field from the principles of general relativity, I should change the mark of 2 on extensibility for that theory to 9. A similar or even greater improvement would result from

success of the mechanistic interpretation to explain the puzzling features of nuclear physics or other now mysterious effects by reference to its novel field. Indeed, this would force the other theories to take on modifications and necessitate a rescoring in connection with simplicity as well.

Finally, and this seems most important, experiments might be performed which bring into evidence new physical entities giving verified status to those features of the mechanistic view that most count against it. The likelihood of such evidence for Weizel's zerons is low, but there are surely alternative models. When such discoveries are made the whole status of our problem is changed because advantage can then be taken of new circuits of empirical verification, and a complete reappraisal will be necessary.

## MATHEMATICS.—*Critical appraisal of the validity of standard techniques of conformal mapping.*[1] JENNY E. ROSENTHAL, Allen B. Du Mont Laboratories, Inc. (Communicated by Richard K. Cook.)

To illustrate the application of conformal mapping to the solution of potential problems in two dimensions, a large number of textbooks (*1, 2, 3, 4*) consider the case of the condenser consisting of two parallel semi-infinite conducting lines raised to different potentials or the mathematically equivalent hydrodynamical problem of the irrotational flow of fluid out of a long channel into a large reservoir. The final answer given by all the authors cited originated with Helmholtz (*5, 6*) in 1868. It will be pointed out here that standard transformation techniques may give infinitely many solutions of which the Helmholtz expression is but one. A discussion is given of various such solutions, and an examination is made of the conditions which determine the acceptability of a given potential function as a solution of the physical problem.

To evaluate the standard (Helmholtz) solution and the techniques which led to its determination, the problem is formulated in terms of the following convenient units:

Consider two semi-infinite lines $y = 1$ and $y = -1$ in the $(x, y)$ plane, which extend from $x = -\infty$ to $x = 0$, with the upper line at a potential $+1$, and the lower one at a potential $-1$. The problem is to find the solution of Laplace's equation

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} = 0 \qquad (1)$$

for these boundary conditions.

[1] A report on the preliminary phases of this work was presented at a meeting of the American Mathematical Society, New York City, February 1951.

The method used to solve such problems by analytical transformations may be summarized as follows:

Let $V$ and $U$ be the electrostatic potential and flux respectively, and let $W = U + iV$. Consider now a problem with a known solution consisting of a set of boundaries in the $(\xi, \eta)$ plane, over which $V$ is constant. Let the known solution be of the form

$$\zeta = f(W), \qquad (2)$$

where $\zeta = \xi + i\eta$. If an analytic transformation

$$z = F(\zeta) \qquad (3)$$

can be found which transforms the boundaries of the known problem to the boundaries of the given problem, then

$$z = F(f(W)) \qquad (4)$$

is the required solution.

In the present instance the obvious choice for the known boundary value problem in the $\zeta$ plane is the case of two infinite lines $\eta = \pm 1$ kept at the potentials $\pm 1$ respectively. In the region between these boundaries the solution has the form

$$\zeta = W, \qquad (5)$$

i. e., the equipotentials constitute a set of infinite straight lines parallel to the given boundaries with the lines of force perpendicular to them.

It will now be shown that

$$z = \zeta + (e^{\pi\zeta} + 1)/\pi \qquad (6)$$

transforms the two infinite lines $\eta = \pm 1$ into two semi-infinite lines $y = \pm 1$ extending from $x = -\infty$ to $x = 0$. Writing the real and imaginary parts separately we obtain

$$x = \xi + (e^{\pi\xi} \cos \pi\eta + 1)/\pi,$$
$$y = \eta + \frac{1}{\pi} e^{\pi\xi} \sin \pi\eta. \tag{7}$$

Hence for $\eta = \pm 1$

$$x = \xi - (e^{\pi\xi} - 1)/\pi,$$
$$y = \pm 1. \tag{8}$$

In Eqs. (8) when $\xi = -\infty$, $x = -\infty$. The value of $x$ increases with $\xi$, up to the point where

$$\frac{dx}{d\xi} = 1 - e^{\pi\xi} = 0, \tag{9}$$

i. e. up to $\xi = 0$. The corresponding maximum value of $x$ is $x = 0$. As $\xi$ increases still further, $x$ decreases and for $\xi = +\infty$ $x = -\infty$. Thus the function (6) essentially folds the lines $\eta = \pm 1$ over along the $\eta$-axis. This folding over is accompanied by a distortion of the metric of the right-hand half of the lines $\eta = \pm 1$ as may be seen from the fact that in Eq. (8) $x$ is not an even function of $\xi$. For the sake of brevity we shall henceforth refer to the distortion of the metric as "puckering".

The function (6) has the added property of transforming the $\xi$-axis into the $x$-axis. (No folding over occurs for this line since for $\eta = 0$, $x = \xi + (e^{\pi\xi} + 1)/\pi$, which is a monotonically increasing function of $\xi$).

Since the function (6) accomplishes the desired transformation of the boundaries, and since $f(W)$ has the form (5), it has been concluded in the past that the required solution is

$$z = W + (e^{\pi W} + 1)/\pi. \tag{10}$$

In carrying out our appraisal of this solution we shall first show that the transformation (6) is not unique and that infinitely many functions can be found which transform two parallel infinite lines into two parallel semi-infinite lines.

Consider a function of the type

$$z = \zeta + \sum_{n=0} C_n e^{n\pi\zeta} \tag{11}$$

where the sum may contain either a finite or an infinite number of terms. In the latter case the series is assumed to be absolutely and uniformly convergent for all finite values of $\zeta$. The real and imaginary parts of Eq. (11) are:

$$x = \xi + \sum_{n=0} C_n e^{n\pi\xi} \cos n\pi\eta$$
$$y = \eta + \sum_{n=0} C_n e^{n\pi\xi} \sin n\pi\eta \tag{12}$$

It is evident that for $\eta = \pm 1$,

$$x = \xi + \sum_{n=0} (-)^n C_n e^{n\pi\xi}$$
$$y = \pm 1 \tag{13}$$

Thus in order to fold the lines $\eta = \pm 1$ along the $\eta$-axis, the transformation must satisfy the following conditions:
At $\eta = \pm 1$

$$x = -\infty \quad \text{for} \quad \xi = \pm\infty \tag{14}$$

$$\frac{dx}{d\xi} = 0 \quad \text{for} \quad \xi = 0, \quad x_{\max} = 0, \tag{14a}$$

$$\frac{dx}{d\xi} \neq 0 \quad \text{for} \quad \xi \neq 0. \tag{14b}$$

At $\eta = 0$, the value of $x$ given by

$$x = \xi + \sum_{n=0} C_n e^{n\pi\xi} \tag{15}$$

must be a monotonically increasing function of $\xi$ ranging from $-\infty$ to $+\infty$ for the same range of variation in $\xi$.

The requirements at $\xi = -\infty$ are satisfied automatically. At $\xi = +\infty$ two different cases have to be considered depending on whether the sum in Eq. (11) contains a finite or an infinite number of terms.

If the number of terms is finite, then the coefficient of the highest order infinity must be negative in Eq. (13) and positive in Eq. (15). Both conditions are satisfied if the highest value of $n$ is odd, $n = 2N + 1$, and if $C_{2N+1} > 0$.

If, on the other hand, the number of terms is infinite, then the following conditions must be satisfied: $\sum_{n=0}^{\infty} C_n e^{n\pi\xi}$ must be monotonically increasing function of $\xi$. Also as $\xi$ tends to $+\infty$, the function $\sum_{n=0}^{\infty} (-)^n C_n e^{n\pi\xi}$ must tend to $-\infty$ more rapidly than $-\xi$.

The derivative at $\eta = \pm 1$ has the form

$$\frac{dx}{d\xi} = 1 + \pi \sum_{n=1} (-)^n n C_n e^{n\pi\xi} \qquad (16)$$

It will vanish at $\xi = 0$ provided

$$1 + \pi \sum_{n=1} (-)^n n C_n = 0 \qquad (17)$$

The corresponding maximum value of $x$ will be 0 if

$$\sum_{n=0} (-)^n C_n = 0 \qquad (18)$$

To satisfy (14b) the right-hand side of Eq. (16) must represent a function with a single zero for the whole range of variation of $\xi$. If the summation extends only to $n = 2N + 1$, this requirement is reduced to a set of inequalities imposed on the various $C_n$. The explicit formulation of these inequalities giving necessary and sufficient conditions can be carried out only for $N \leq 2$, but sufficient conditions can be found for all values of $N$.

It is obvious that there are infinitely many ways of choosing the coefficients $C_n$ so as to satisfy the various conditions stated above. Examples chosen at random are given below. It will be noticed that some of them contain arbitrary parameters.

The value $N = 0$ corresponds to the Helmholtz solution.

If $N = 1$, then all conditions for a suitable transformation are satisfied by the function

$$z = \zeta + C_0 + (C_3 + 2C_0 - 1/\pi)e^{\pi\zeta} + (2C_3 + C_0 - 1/\pi)e^{2\pi\zeta} + C_3 e^{3\pi\zeta} \qquad (19)$$

provided the coefficients $C_0$ and $C_3$ fall within one of the following three categories:

$$\begin{cases} \pi C_0 < 2.5 \\ 8 - 2\pi C_0 - (60 - 24\pi C_0)^{\frac{1}{2}} < \pi C_3 \\ \qquad < 8 - 2\pi C_0 + (60 - 24\pi C_0)^{\frac{1}{2}}; \end{cases} \qquad (20)$$

or

$$\pi C_0 = 1\tfrac{1}{6}, \qquad \pi C_3 = \tfrac{1}{3}; \qquad (21)$$

or

$$0 < \pi C_3 < 2 - 2\pi C_0 . \qquad (22)$$

If the summation extends to an infinite number of terms, then a suitable example is provided by the function

$$z = \zeta - \left\{ e^{-k} \sum_{n=0}^{\infty} (-)^n \frac{k^n}{n!} e^{n\pi\zeta} - 1 \right\} \Big/ \pi k = \zeta - \{ e^{-k(1+e^{\pi\zeta})} - 1 \}/\pi k, \quad \text{with} \quad k > 0. \qquad (23)$$

The general solution to the potential problem corresponding to the transformation (11) is

$$z = W + \sum_{n=0} C_n e^{n\pi W} \qquad (24)$$

At $V = \pm 1$

$$x = U + \sum_{n=0} (-)^n C_n e^{n\pi U}, \qquad y = \pm 1 \qquad (25)$$

As pointed out by Jeans (*1*), for each value of $x$ there are two values of $U$, one negative, one positive; $U < 0$ corresponds to the inside of the condenser and $U > 0$ to the outside. These remarks are helpful in drawing some general conclusions as to the electric charge on the condenser. According to Gauss' theorem, the charge contained within any surface is found by integrating over the whole surface the component of the electric intensity directed along the outward normal to the surface. The expression for the electric intensity is

$$-\left|\frac{dW}{dz}\right| = \frac{-1}{\left|1 + \sum_{n=1} n\pi C_n e^{n\pi W}\right|} \qquad (26)$$

At the condenser lines, i. e., when $V = \pm 1$, it takes the form

$$-\left|\frac{dW}{dz}\right| = -\frac{1}{\left|1 + \sum_{n=1} (-)^n n\pi C_n e^{n\pi U}\right|}. \qquad (27)$$

Since Eq. (27) does not represent an even function of $U$, the electric intensity at the condenser lines for any given value of $x$ has different values on the inside and on the outside of the condenser. Thus the charge has different values along the inside and the outside of the condenser lines. However, while the electric intensity has the same two

values on the upper as on the lower condenser line, it is directed along the outward normal to the upper line and along the inward normal to the lower line. Therefore the charges along the two condenser lines are equal and opposite, and the total charge on the condenser is zero for all the functions considered. A comparison of Eq. (27) with Eq. (16) shows that

$$\left|\frac{dW}{dz}\right| = \frac{1}{\left|\frac{dx}{dU}\right|} \tag{28}$$

Using Eqs. (14a) and (14b) (which in view of Eq. (5) hold for $dx/dU$ as well as for $dx/d\xi$) we may conclude that at $x = -\infty$ the charge density is finite on the inside of the condenser and zero on the outside and that at $x = 0$, i. e. at the edge of the condenser lines the charge density is infinite. This result again holds for all functions considered.

The Helmholtz function (10) is next analyzed with regard to its behavior at infinity, and the results are compared with what would be expected on the basis of physical considerations. To avoid any difficulties with potential, i. e. charge, distributions of infinite length, we start with the case where the two conducting lines at different potentials have finite length. Let the two lines $y = 1$ and $y = -1$ (kept at the potentials $+1$ and $-1$ respectively) extend from $x = -L$ to $x = 0$, $L$ being a large number. For this distribution when $x$ and $y$ are infinite the potential is zero by definition. Consider now the potential distribution along a line $x = -L'(0 < L' < L)$ extending from $y = -\infty$ to $y = \infty$. At $y = 0$ the potential on this line is 0. As $y$ increases from 0 to 1, the potential along the line also increases from 0 to 1. As $y$ then increases from 1 to $\infty$, the potential decreases from 1 to 0. (Similarly, the potential along the negative portion of this line goes from 0 to $-1$ to 0 as $y$ varies from 0 to $-1$ to $-\infty$.) As $L$ is increased, the potential distribution along $x = -L'$ maintains these general features. In particular every potential value between 1 and 0 is encountered in the range $1 \leq y \leq \infty$. There is no possibility of any discontinuity arising which will change these general characteristics when we go to the limit and let $L$ tend to infinity since the total charge on the condenser is zero.

In the problem of two semi-infinite conducting lines at the potentials $+1$ and $-1$ respectively we therefore expect the following behavior of the equipotentials:

Every equipotential line should start at $x = -\infty$ in the region between the conducting lines, then, at some point in the right half-plane, turn around the line of the same sign and go back to $x = -\infty$. The only exception is the zero equipotential, which—as may be seen from symmetry considerations—extends in a straight line from $x = -\infty$ to $x = +\infty$. This behavior is illustrated in Fig. 1 for the equipotential lines $V = \pm 5/6$ calculated from Eq. (10).

However, the equipotential lines defined by Eq. (10) show these characteristics only for $|V| > 1/2$. For $|V| \leq 1/2$, $x$, as defined by Eq. (10), is a monotonically increasing function of $U$ (see Eqs. (7)). Thus for $|V| \leq 1/2$ the equipotential lines do not go back to $x = -\infty$ but proceed out to $x = +\infty$. The immediate consequence of

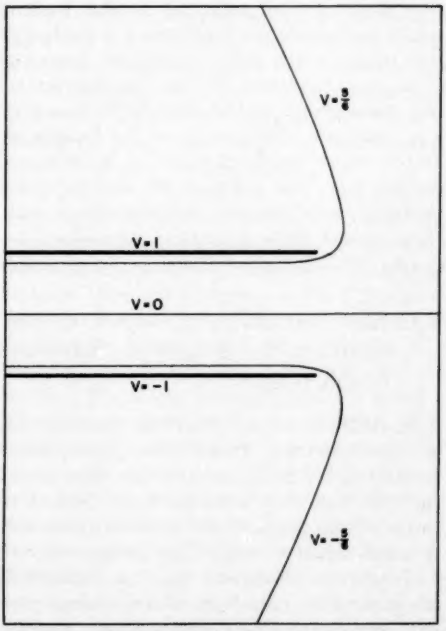


FIG. 1.—General behavior of equipotential curves.

this division of equipotential lines into two classes with different characteristics is as follows:

As $y$ goes from 1 to $\infty$, the potential along the line $x = -L'(0 < L' < \infty)$ no longer varies from 1 to 0 but only from 1 to $\frac{1}{2} + \epsilon$ with $\epsilon > 0$. Similarly, as $y$ goes from $-1$ to $-\infty$ along the same line, the potential varies from $-1$ to $-\frac{1}{2} - \epsilon$. Thus the potential at an infinite distance from the conducting lines is not a unique constant but assumes different values in different portions of the infinite domain even though the total charge is zero.

The reason for the discrepancy in the behavior of the equipotential lines calculated from Eq. (10) and that to be expected from physical considerations may lie in the nature of the mapping. As mentioned in the beginning, the $z$-plane is obtained by mapping—by means of function (6)—the infinite strip of the $\zeta$-plane bounded by $\eta = \pm 1$. The basic solution of the potential problem in the $\zeta$-plane is valid only in the region between the conducting lines and does not hold for $|\eta| > 1$. No consideration is given to values of the potential in the $\zeta$-plane which lie outside the conducting lines though this problem has definite physical meaning.

Since all functions of type (11) accomplish the same result as the Helmholtz function, i. e., map an infinite strip of the $\zeta$-plane on to the whole of the $z$-plane, it is to be expected that the solution of the potential problem based on any of these transformations would show a similar anomalous behavior. This is indeed the case. For example for any solution of type (24) extending to $n = 2N + 1$, the equipotential lines are divided into two different classes by the limiting value

$$|V| = \frac{1}{2(2N+1)}. \qquad (29)$$

(The Helmholtz solution corresponds to the special case $N = 0$). However, the limiting value is not a continuous function of $N$, for as $N$, i. e. the number of terms in the series, becomes infinite, the limiting value does not decrease to 0 as might be expected from Eq. (29). For example, the limiting value for

$$z = W - \{e^{-k(1+e^{\pi W})} - 1\}/\pi k \qquad (30)$$

obtained from transformation (23) is $|V| = \frac{1}{2}$, as in the case of Eq. (10).

It would seem therefore that a transformation which maps only the strip between the conducting lines in the $\zeta$-plane on to the whole of the $z$-plane, while at the same time folding over the conducting lines, is not likely to lead to a physically meaningful solution.

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PALEONTOLOGY.—*Two new crinoid species from the Henryhouse of Oklahoma.* HARRELL L. STRIMPLE, Bartlesville, Okla. (Communicated by Alfred R. Loeblich, Jr.)

In 1952 the author described three species of *Lecanocrinus* from the Henryhouse formation (Silurian) and at the time noted (p. 318) that they were the most distinctive forms of the genus found in that formation. A small species, somewhat comparable to *L. pisiformis* (Roemer), was not considered at that time pending closer comparison with the Beech River (Brownsport) species. Several specimens of *L. pisiformis* from the vicinity of Decatur, Tenn., were found in the collections made by the author and his wife, Mrs. Melba Strimple, during the years 1951 and 1952. The Beech River form is very close to the Henryhouse species but lacks the strong papillae and has a different arm development. The name *L. papilloseous*, n. sp., is proposed for the small form found in the Henryhouse formation.

A unique species of *Pisocrinus* from the Henryhouse formation is described below as *P. spatulatus*, n. sp. It is from the same horizon that produces *L. papilloseous*.

SAGENOCRINOIDEA Springer

SAGENOCRINITIDAE Bassler

**Lecanocrinus** Hall

**Lecanocrinus papilloseous,** n. sp.

FIGS. 1–4, 9, 10

The crown is spherical in outline, with the dorsal cup occupying almost two-thirds of the height of the crown. Greatest width is at about midsection of the basal circlet and is almost equal to the height of the crown.

Dorsal cup is wider than high and is composed of three small IBB, five relatively small BB, five very large RR, a small quadrangular shaped RA, and a narrow, elongated anal X. The IBB circlet is almost entirely covered by the large, round stem scar and is barely visible in side view of the dorsal cup. The smaller IB is right posterior in position.

There are 10 broad, short, asymmetrical arms. Second primibrachials are axillary in all rays. The widest arms and largest secundibrachials are in the left anterior ray. The other rays are of smaller, and approximately equal width at their base but are unequal in their distal portions. The rays of the right posterior is better developed than the right anterior ray, and has the greatest number of secundibrachials of any ray. The upper portions of the left posterior and interior rays are very restricted in size.

The entire crown is ornamented with minute papillae that form no pattern, and on occasion appear to coalesce.

*Measurements in mm.*—As follows:

| | Holotype |
|---|---|
| Height of dorsal cup | 8.0 |
| Height of crown | 10.0 |
| Maximum width of crown | 8.7 |

*Remarks.*—This species is more comparable to *Lecanocrinus pisiformis* (Roemer) than to other described species. *L. pisiformis* is a smaller form yet has more secundibrachials (3–4 SBrBr to a ray), which are symmetrical as they diminish in size. In *L. papilloseous* there are 1 to 3 SBrBr to a ray and they are very irregular in size and shape.

*L. invaginatus* Strimple, which is also from the Henryhouse formation, is a slightly larger form that is somewhat similar in so far as general cup outline is concerned. The only surface sculpture on *L. invaginatus* is a fine frosted appearance. The arms are much larger, occupy a considerably greater portion of the crown, and have more than one bifurcation, which is quite different from the arm structure of *L. papilloseous*.

Other associated species of *Lecanocrinus* from the Henryhouse Formation are quite distinct from *L. papilloseous* and are not found in the "Pisocrinus" horizon that produces this species.

*Occurrence.*—Holotype collected by the author in SW/4 NW/4 NW/4 section 33, T.3N., R.6E., figured paratype in NW/4 SW/4 section 4, T.2N., R.6E., Pontotoc County, south of Ada, Okla.; upper Henryhouse formation, Silurian.

*Types.*—To be deposited in the U. S. National Museum.

DISPARATA Moore and Laudon

PISOCRINIDAE Angelin

**Pisocrinus** de Koninck

**Pisocrinus spatulatus,** n. sp.

FIGS. 5-8

This species is represented in the collections of the author by three dorsal cups all found within a yard of one another, on one field excursion, in a fresh excavation. Subsequent searching has failed to produce any additional specimens referable to the species though other species of *Pisocrinus* are rather common in the exposure.

In general plate structure, the form does not differ appreciably from several species of the genus; there are five asymmetrical BB, the smallest being in the right posterior radius; one large plate in the right posterior, which I prefer to term the radianal rather than inferradianal as proposed by Moore and Laudon (1943, p. 27); and five asymmetrical RR. I am unable to see the need for calling the right posterior radial a superradianal as proposed by Moore and Laudon (1943, p. 27) when the plate is in fact supporting an arm, the same as any other radial plate. A large spatulate shaped extension to the fore of the outer ligament furrow, gives the cup a distinctive appearance when viewed from any direction that is not comparable to any other described species. Small clusters of minute nodes are found on some of the plates in profusion.

The arms, tegmen, and column are not known.

The columnar scar is small, circular in outline and reposes at the base of a small basal invagination.

*Measurements in mm.*—As follows:

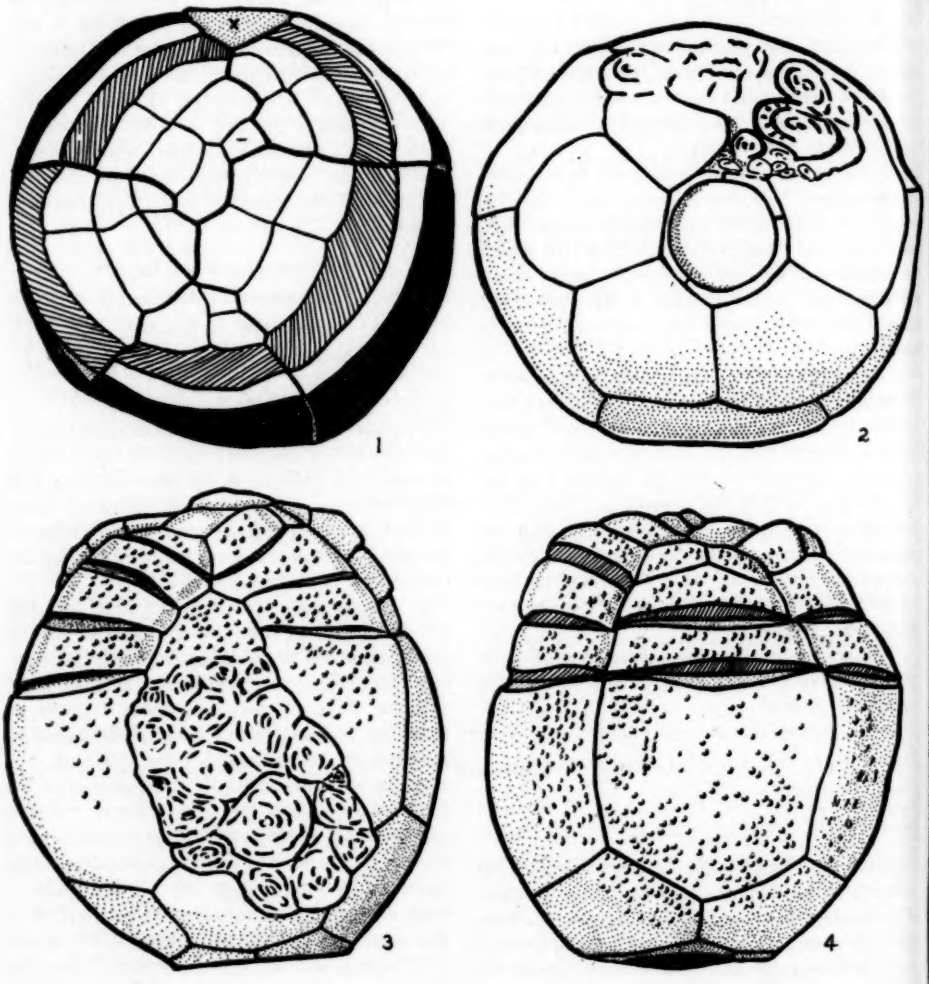
| | Holotype |
|---|---|
| Maximum width of dorsal cup (including the horizontal extensions of RR) | 6.9 |
| Height of dorsal cup (to transverse ridge of articulating facets) | 2.6 |

*Occurrence.*—NW/4 SW/4 section 4, T.2N., R.6E., Pontotoc County, south of Ada, Okla.; upper Henryhouse formation, Silurian.

*Types.*—To be deposited in the U. S. National Museum.

## REFERENCES

All references are to be found in Bassler and Moodey, 1943. *Bibliographic and faunal index*

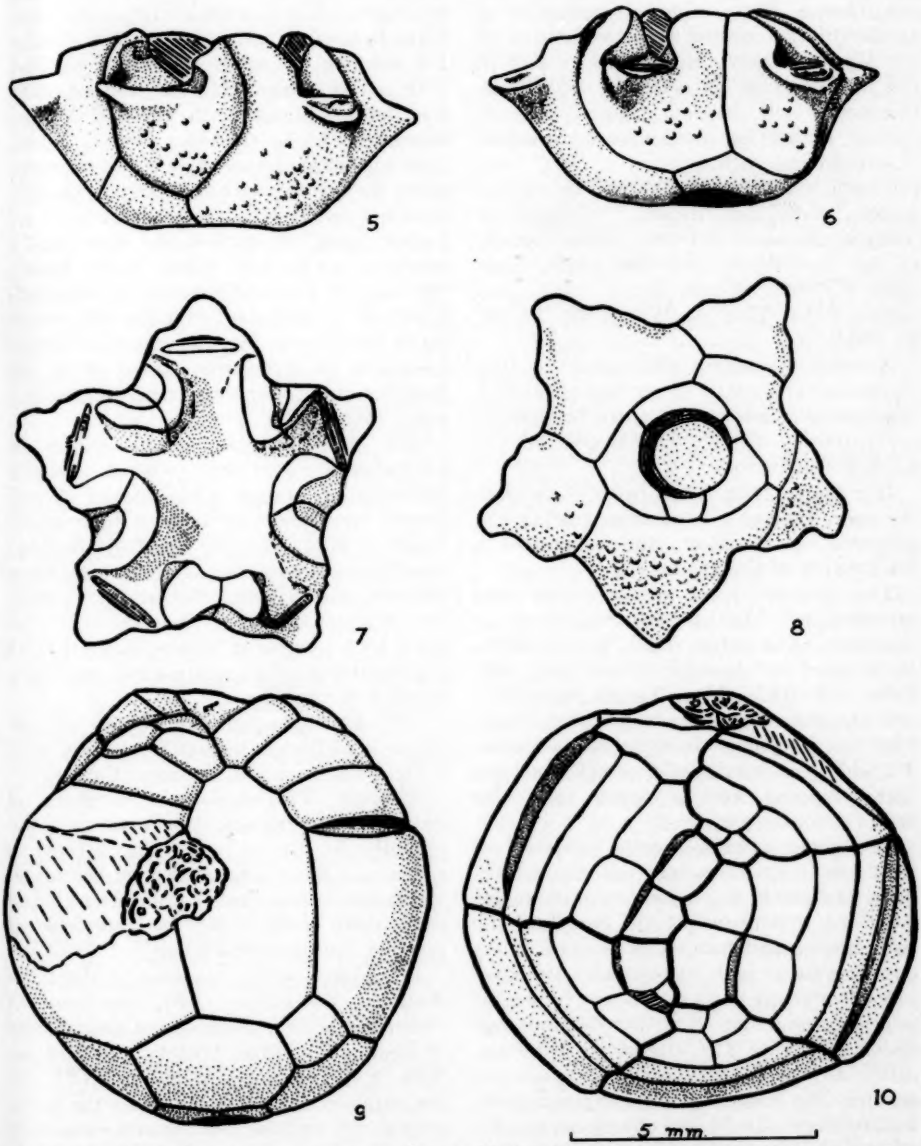



FIGS. 1–4.—*Lecanocrinus papilloseous*, n. sp. Camera-lucida drawings of the holotype from the summit, base, posterior and anterior. In the summit view, radial plates are shown in solid black, axillary primibrachials shown by diagonal markings and the posterior interradius (anal X) shown by stippling.

*of Paleozoic echinoderms*, Geol. Soc. Amer., Special Paper no. 45, with the following exceptions:

MOORE, RAYMOND C., and LAUDON, LOWELL R., *Evolution and classification of Paleozoic crinoids*, Geol. Soc. Amer. Special Paper no. 46. 1943.

STRIMPLE, HARRELL L. *New species of* Lecanocrinus. Journ. Washington Acad. Sci. 4(10): 318–323. 1952.




FIGS. 5–8.—*Pisocrinus spatulatus*, n. sp. Camera-lucida drawings of holotype from anterior, posterior, summit, and base.

FIGS. 9, 10.—*Lecanocrinus papilloseous*, n. sp. Camera-lucida drawings of a paratype from the posterior and summit.

BOTANY.—Centrolobium (*Leguminosae*): *Validation of a specific name and a brief review of the genus.* VELVA E. RUDD, U. S. National Museum. (Communicated by Jason R. Swallen.)

In October 1918, Dr. J. N. Rose and his son, George Rose, collected specimens of an Ecuadorian timber tree, a member of the legume family, known locally and in the lumber trade as *amarillo*, *amarillo de Guayaquil*, and *amarillo lagarto*. The resulting herbarium sheets were inscribed "Centrolobium ochroxylum, n. sp.," and the name was subsequently used by various writers, including Record (Timbers of Tropical America, 291–293. 1924; Timbers of the New World, 242–243. 1943), Rimbach (Tropical Woods **31:** 4. 1932), and Acosta Solis (Tropical Woods **89:** 12, 23, 33. 1947).

A valid taxonomic description of that species has apparently never been published, a situation which was recently brought to my attention by W. A. Dayton, of the U. S. Forest Service.

It is the purpose of this paper to validate the specific name *Centrolobium ochroxylum*, proposed by Dr. Rose, and to present a brief review of the genus.

The generic name *Centrolobium* was proposed by Martius and published by Bentham (Ann. Mus. Vind. **2:** 95. 1838). It is based on *Nissolia robusta* Vell. (Fl. Flum. 298. 1825. Icon. **7:** tab. 85. 1835), and, therefore, *Centrolobium robustum* (Vell.) Mart. ex Benth. is the type of the genus. Six additional species of *Centrolobium* have been proposed, two of which have also been treated as varieties.

The genus *Centrolobium* is composed of trees about 10–30 meters tall, the trunks being as much as 1 meter in diameter, commonly buttressed at the base and the bark grayish and smooth, or fissured. When cut, the stems and roots exude a reddish sap. Most young parts are densely brownish-pubescent and dotted with reddish-orange resinous punctae. The stipules are caducous, deltoid to broadly orbiculate, acute, about 1–2 cm long. The leaves are large, imparipinnate, 7-21-foliolate, the leaflets oblong-ovate, acuminate to obtuse, entire, punctate below, pubescent to glabrous, membranaceous to subcoriaceous, pinnately veined, the costa essentially central. The bracts are stipule-like but smaller. The flowers are papilionaceous, borne in large terminal panicles, the corollas 1–2 cm long, yellowish, sometimes suffused with red or violet, glabrous, the standard spatulate or obovate, with callosities near the base of the blade, the calyx densely pubescent, turbinate-campanulate with 4 subequal lobes, the lobe opposite the standard emarginate or 2-parted. The fruits are large, 1–3(–4)-seeded, samaroid legumes, the style usually persisting as a stout spine (stylar spine), the body of the fruit spherical or elliptical, 1.5–5 cm in diameter, echinate with spines up to about 4 cm long, the wings obliquely spatulate or cultriform, about 5–18 cm long measured along the median longitudinal axis, 2–9 cm wide.

The wood of the various species of *Centrolobium* is richly colored, usually yellowish or orange, with streaks of red, purple, or black, and takes a high satiny finish. It is of value for furniture, flooring, cooperage, shipbuilding, railway ties, mine timbers, and general construction. According to Record (op. cit.) it is known to the trade by a number of names, in the United States principally as canary wood, porcupine wood, and zebra wood.

The known geographic range of the genus is restricted to South America, with a slight extension into Panama (Fig. 1).

Although *Centrolobium* is a genus of economic importance, its occurrence apparently is not too common within its range, and the representation by herbarium specimens is remarkably poor. It is hoped that more collections, as complete as possible, will be forthcoming.

In addition to the material at the U. S. National Herbarium (US), specimens of *Centrolobium* were examined at the Chicago Museum of Natural History (F) and the New York Botanical Garden (NY). To the curators of those institutions the writer wishes to express her thanks for their courtesy. The initials of the herbaria, as cited, follow those given by Lanjouw and Stafleu (Index Herbariorum, 1952).

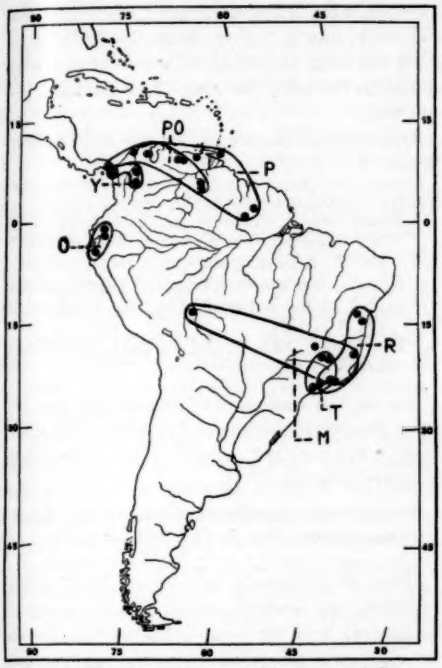



FIG. 1. The known geographic range of *Centrolobium* (M—*C. minus*; O—*C. ochroxylum*; P—*C. paraense* var. *paraense*; PO—*C. paraense* var. *orinocense*; R—*C. robustum*; T—*C. tomentosum*; Y—*C. yavizanum*).

The following key to species, based on admittedly inadequate material, is intended to facilitate comparison of the previously described taxa as well as to place the species which is described as new in this paper.

KEY TO SPECIES OF CENTROLOBIUM

Stylar spine (persistent style) with the wing adherent almost to the apex, or the spine weak and not more than 1 cm long; leaves (9–)13–21-foliolate, the leaflets commonly ovate-oblong, not more than 6 cm wide.
  Stipe of fruit about 2.5 cm long; leaflets submembranaceous (Panama; Colombia) 4. *C. yavizanum*
  Stipe of fruit less than 2 cm long; leaflets chartaceous or subcoriaceous.
    Fruit essentially sessile; pedicels 3–5 mm long, 2–3 mm thick (southeastern Brazil) 3. *C. tomentosum*
    Fruit stipitate, the stipe (6–)8–15 mm long; pedicels 5–10 long, 1–2 mm thick.
      Body of fruit 4–8 cm long, 3–5 cm broad, the spines fairly rigid, mostly 2–4 cm long, the stylar spine 2.5–5 cm long; wing 12.5–16 cm long, 6–9 cm wide (southeastern Brazil) ..... 1. *C. robustum*
      Body of fruit 2.5–3 cm long, 1.5–2.5 cm wide, the spines fine, rather weak, commonly less than 2 cm long, the stylar spine 1–2 cm long, often inconspicuous; wing 6–13 cm long, 2–6 cm wide (eastern Bolivia; southeastern Brazil) ..... 2. *C. minus*
Stylar spine free from the wing except for basal portion 1 cm long or less; leaves 7–15-foliolate, the leaflets ovate-elliptic, frequently as much as 10–14 cm wide.
  Apex of stylar spine recurved (Ecuador) 5. *C. ochroxylum*
  Apex of stylar spine ascending or perpendicular to the longitudinal axis of the wing.
    Leaflets subcordate, pubescent, sometimes glabrescent (British Guiana; eastern Venezuela; northern Brazil) 6a. *C. paraense* var. *paraense*
    Leaflets rounded at base to subcordate, subglabrous or glabrate (Panama; Trinidad, cultivated ?; Venezuela; Colombia) 6b. *C. paraense* var. *orinocense*

1. **Centrolobium robustum** (Vell.) Mart. ex Benth. Ann. Mus. Vind. **2**: 95. 1838.
*Nissolia robusta* Vell. Fl. Flum. 298. 1825. Icon. **7**: tab. 85. 1835.
*Centrolobium robustum* var. *macrochaete* Mart. ex Benth. in Mart. Fl. Bras. **15**[1]: 263. 1862.

Tree about 30 meters tall; leaves (9–)13–21-foliolate, the petiole and rachis subglabrous, slender, commonly less than 5 mm thick at base of petiole, the leaflets chartaceous to subcoriaceous, moderately pubescent to subglabrous, ovate-oblong, 4–10 cm long, 1.5-6 cm wide, acuminate, the base obliquely rounded to subcordate; pedicels about 5–6 mm long and 1–1.5 mm thick; legume with stipe 1 cm long, the body of the fruit 4–8 cm long, 3-5 cm broad, the spines fairly rigid, mostly 2–4 cm long, the stylar spine 2.5–5 cm long, ascending, adherent to the wing along 2 cm of its length or more, any protruding portion weak and easily broken, the wing 12.5–16 cm long, 6–9 cm wide.

Distribution: Known only from Brazil, in eastern Bahia south to northern São Paulo.

BRAZIL: (Local name, *putumuju*)
Bahia: [Grungogi] "Grongogy basin", *Curran* 296 (US). Between "Jiquey e Iracema," *Frões* 20171 (NY).
Rio de Janeiro or Distrito Federal: *Vellozo* (Icon. **7**: tab. 85, presumably based on type); *Riedel & Luschnath* 448 (US). Macahe, *Riedel & Luschnath* 464 (NY, US).
São Paulo: Ubatuba, *Guillemin* 592 (F).

Included in this species are the collections with the largest fruits and the smallest leaflets.

2. **Centrolobium minus** Presl, Bot. Bemerk. 61. 1844.
*Centrolobium robustum* (Vell.) Mart. ex Benth. var *microchaete* Mart. ex Benth. in Mart. Fl. Bras. **15**¹: 263. 1862.

Tree, about 30 meters tall, the trunk about 8 dm in diameter; leaves 13–17-foliolate, the petiole and rachis moderately pubescent to subglabrous, slender, commonly less than 5 mm thick at the base of the petiole, the leaflets subcoriaceous, moderately pubescent to subglabrous, ovate to oblong, 6–10 cm long, 3–5 cm wide, acuminate to obtuse, the base rounded, sometimes obliquely; pedicels 8–10 mm long, 1–2 mm thick; legume with stipe (6–)8–15 mm long, the body of the fruit 2.5–3 cm long, 1.5–2.5 cm wide, the spines fine, rather weak, 1–2 cm long, the stylar spine 1–2 cm long, adherent to the wing along its entire length or nearly so, often rather inconspicuous, the wing 6–13 cm long, 2–6 cm wide.

Distribution: Southeastern Brazil and eastern Bolivia.

BOLIVIA:
La Paz: La Asunta, *Krukoff* 10625 (NY).
BRAZIL: (Local names, *araribá, lei nova.*)
Espirito Santo: Collantina, *Whitford & Silveira* 72 (US).
Minas Gerais: Capoeira, *E. de Oliveira,* May 6, 1941 (US). Fazenda da Cachoeira, Munic. Tombos, *Mello Barreto* 1509 (F), 1691 (F).
Rio de Janeiro or Distrito Federal: *Luschnath* (tab. 74, Presl. Symb. Bot. 1858, presumably based on type).

Although Martius and Bentham treated this taxon as only varietally distinct from *C. robustum,* I believe that it is more closely related to. *C. tomentosum.* For the time being I prefer to consider it as specifically distinct and thus to use Presl's designation of *C. minus.*

3. **Centrolobium tomentosum** Benth. in Hook. Journ. Bot. **2**: 66. 1840.

Tree, 20 meters tall or more; leaves 13-17-foliolate, the petiole and rachis pubescent, 5 mm thick or more at base of petiole, the leaflets ovate to elliptic, 8–15 cm long, 4.5–5.5 cm wide, obtuse to acuminate, the base rounded to subcordate, usually oblique, the upper surface pubescent or sometimes glabrate, the lower surface pubescent; pedicels about 3–5 mm long and 2–3 mm thick; legume essentially sessile, the body of the fruit 3.5–4 cm long, 2.5–3.5 cm thick, the spines fine and weak, mostly 1–2 cm long, the stylar spine 3.5–4 cm long, ascending, adherent to the wing almost to the apex, the wing 12–15 cm long, 6–7 cm. wide.

Distribution: Known only from southeastern Brazil.

BRAZIL: (Local names, *araribá, araribá rosa.*)
Minas Gerais: *Claussen* (photo of type ex K, NY negative, new series no. 2677); *Claussen* 879 (F, NY, isotypes ?).
Rio de Janeiro or Distrito Federal: *Sellow* (NY, US). Botanical Garden, *Whitford* 10 (F, NY, US).
São Paulo: São Paulo, in park, *Kuhlmann* 31626 (NY).

Among the specimens of this species are the most densely tomentose of the genus. The character is not constant, however, and some collections are glabrate.

4. **Centrolobium yavizanum** Pittier, Journ. Washington Acad. Sci. **5**: 459. 1915.

Tree, 25–30 meters tall; leaves 13–17(–19?)-foliolate, the leaflets (on basis of immature specimens) 6–12 cm long, 3–5.5 cm wide, ovate to elliptical, pubescent, possibly glabrescent, acuminate, rounded to subcordate at base; mature flowers not seen; legume with stipe about 2.5 cm long, the body of fruit 3–3.5 cm long, 2–2.5 cm wide, the spines fine and weak, 1–2 cm. long, the stylar spine 1 cm long or less, adherent to wing along about half its length or more, the apical portion free, ascending or perpendicular to long axis of wing, the wing 10–12 cm long, 4.5–5.5 cm wide.

Distribution: In forest, southern Panama and northern Colombia.

PANAMA:
Darien: Between Pinogama and Yaviza, *Pittier* 6572 (NY, US, type).
COLOMBIA: (Local name, *guayacán jobo.*)
Bolívar: Lands of Lobo, *Curran,* in Apr.-May 1916 (US). ? Puerto Berrio, *Haught* 1688 (NY).

The fruits of the first two collections cited differ slightly in wing shape, but in other particulars, including stipe length, which I believe to be a critical diagnostic character, they are essentially the same. The *Haught* specimen, with one leaf and an immature inflorescence, is placed here tentatively on the basis of the shape and number of leaflets.

5. **Centrolobium ochroxylum** Rose ex Rudd, sp. nov. FIG. 2

Arbor, decidua, 10–30 m alta; ramuli novelli fulvo-tomentosi, demum glabrescentes, cortice griseo, tronco 5 dm diametro; folia 7–13-foliolata, imparipinnata; foliola ovata, 6–20 cm longa, 5–14 cm lata, apice acuminata vel acuta, basi rotundata vel subcordata, margine integra, foliola terminali saepe maxima, jugis inferioribus brevioribus, tomentosa, glabrata, subtus resinoso-punctata, punctis subrufis vel aureis; flores non vidi; legumen sphaericum, 3-4 cm diametro, dense spinosum, spinis 2–3 cm longis, stipite 1 cm longo, styli vestigio 1.5–2 cm longo, indurato, apice recurvato, ala obliquo-spathulata, 14–17 cm longa, 7–10 cm lata, tomentosa, glabrescente, glanduloso-punctata.

Tree, 10–30 meters tall, deciduous; stems brownish-tomentose when young, glabrescent; mature trunks as much as 5 dm in diameter, the bark gray; stipules caducous, ovate, acute, about 6 mm long, 8 mm wide, tomentose, resinous-punctate; leaves 7–13-foliolate, imparipinnate, the lateral leaflets subopposite, the axis striate, tomentose, glabrescent, 25–45 cm long, the petiole 10–15 cm long; leaflets 6–20 cm long, 5–14 cm wide, the terminal leaflet often the largest, the basal pair the smallest, ovate, entire, acuminate or acute, the base rounded to subcordate, the upper surface tomentose, glabrate, the lower surface tomentose, glabrate, resinous-punctate with orange-colored dots, the venation pinnate with about 12–16 major secondary veins on each side of the central costa; flowers not seen; legume spherical, 3–4 cm in diameter, spiny, the spines mostly 2–3 cm long, the stipe about 1 cm long, the stylar spine about 1.5–2 cm long, recurved, the wing obliquely spatulate 14–17 cm long, 7–10 cm wide, tomentose, glabrescent, resinous-punctate.

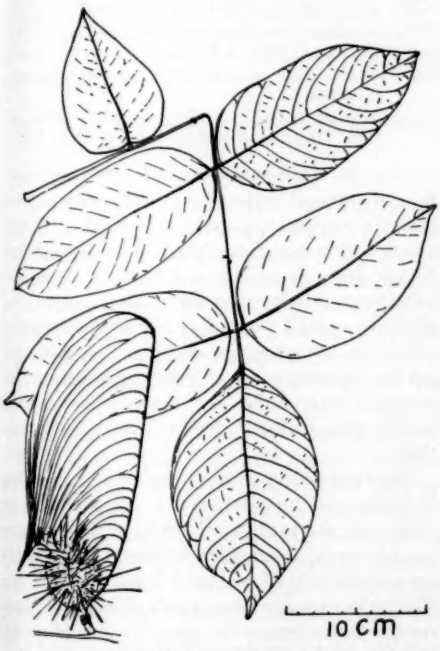



FIG. 2. *Centrolobium ochroxylum* (leaf and fruit, *Rose & Rose* 23370).

Type in the U. S. National Herbarium, no. 1022875, collected at Portovelo (near Zaruma), Province de Oro, Ecuador, October 6–15, 1918, by J. N. Rose and George Rose (no. 23370) Duplicate at NY.

Additional specimens examined:

ECUADOR: (Local names, *amarillo, amarillo de Guayaquil, amarillo lagarto.*)
Guayas: Milagro, *Johansen* 12 (US).
El Oro: Portovelo, *Hitchcock* 21265 (US). Junction of Río Ambocas and Río Luis, 10 km south of Portovelo, *Camp* E-585 (NY). Piedras, *Little* 6615 (US).
Los Ríos: Pichilingue, *Little* 6486 (F, US). Quevado, *Little* 6557 (US).

It is believed by some botanists that *Centrolobium* in Ecuador should be referred to *C. patinense* Pittier, originally described from Panama (in this paper, reduced to synonymy under *C. paraense* var. *orinocense*), and it has been so cited (Holdridge et al., U. S. For. Serv. Publ. 25, 49, 72. 1947). Others have chosen to give it a non-committal designation as *Centrolobium* sp. In the present state of knowledge of the genus and the range of variation of its taxa, it seems desirable to maintain a separate species for the Ecuadorian representatives of *Centrolobium*, although they undoubtedly are very closely related to *C. paraense*.

Acosta-Solis (Tropical Woods **89:** 23. 1947), in a list of commercially valuable woods in the Province of Esmeraldas, Ecuador, includes "Amarillo Lagarto Obscuro (*Centrolobium ochroxylum*)" and "Amarillo Lagarto Claro (*Centrolobium ochroxylum*, var.)" I have not as yet seen any material from Esmeraldas, nor do I know what might be referable to two such varieties.

6. **Centrolobium paraense** Tul. Arch. Mus. Par. **4**: 87. 1844.

Tree, about 30 meters tall, the mature trunk as much as 1 meter in diameter; leaves 7–15-foliolate, the leaflets 7–24 cm long, 5–14 cm wide, ovate to elliptic, obtuse to acuminate, the base rounded to cordate, the upper surface tomentose, often glabrescent, the lower surface sparingly pubescent to subglabrous; flowers about 12–17 mm long, in terminal panicles; bracts densely brown-tomentose, obovate-rhombic, 8–10 mm long, 6–10 mm wide, acuminate, the bracteoles linear, acute, 6–10 mm long, 1–2 mm wide; calyx brown-tomentose, 10–17 mm long; petals yellow, glabrous, the standard broadly spatulate, about 12–17 mm long, 10 mm wide, retuse, the wings and keel 10–14 mm long; stamens about 10 mm long; fruit with stipe 10–15 mm long, the body of the legume 3.5–5 cm long, 2–3.5 cm wide, the spines 1.5–3 cm long, fine, moderately rigid, the stylar spine about 1.5–2.5 cm long, adherent to the wing only along the basal 7–12 mm, the apex free, ascending or perpendicular to the long axis of the wing, the wing 13–18 cm long, 6–9 cm wide.

6a. **Centrolobium paraense** var. **paraense.** *Centrolobium paraense* Tul. Arch. Mus. par. **4**: 87. 1844.

Leaves 7–15-foliolate, the leaflets subcordate, tomentose, sometimes glabrescent, often revolute, the terminal leaflet often conspicuously broader and the basal laterals noticeably smaller than average.

Distribution: Trinidad (cultivated?), eastern Venezuela, British Guiana, and northern Brazil.

TRINIDAD: (Local name, porcupine tree.) Port of Spain, Botanic Garden, *Fairchild* 2844 (US); *Mell.*, Aug. 10, 1923 (NY, US).

BRITISH GUIANA: (Local names, *shipuradai*, *kartang*.) "Pirara, &c," *Schomburgk* 314 (F; isotype; photo of isotype ex G, Field Mus. negative no. 28033). Kanuka Mts., *A. C. Smith* 3207 (F, NY, US). Rupununi R., *Forest Service Brit. Guiana* WB 138 (NY).

VENEZUELA: (Local names, *cartán*, *balaustre*.) Anzoátegui or Sucre: Between Guanta and Los Altos, *Tamayo* 2106 (US). Along Río Querecual, sw of Bergantín, *Steyermark* 61487 (F).

BRAZIL: (Local names, *pão-rainha*, *pau rainha*.) Rio Branco: Serra Grande, *Ducke* 516 (F, NY, US). Between Bôa Vista and Caracarahy, *Frões* 23055 (US).

6b. **Centrolobium paraense** var. **orinocense** Benth. in Mart. Fl. Bras. 15¹: 266 (as *orenocense*). 1862.
*Centrolobium patinense* Pittier, Journ. Washington Acad. Sci. **5**: 470. 1915.
*Centrolobium orinocense* (Benth.) Pittier, Bol. Tecn. M.A.C. Serv. Bot. Caracas **5**: 123. 1944.

Leaves 9–15-foliolate, the leaflets rounded to subcordate, often glabrate, usually fairly uniform in size and shape.

Distribution: In forest, southern Panama, northern Colombia, southeastward to Orinoco region of Venezuela.

PANAMA: (Local name, *amarillo de Guayaquil*.) Darien: Punta Patiño, *Pittier* 6611 (US, type of *C. patinense*).

VENEZUELA: (Local names, *cartán*, *balaustre*.) Guárico: Between Uberito and San Juan de los Morros, *Pittier* 12240 (F, NY, US). Aragua: Between Villa de Cura and San Juan de los Morros, *Pittier* 11358 (NY, US). Zulia: San Martín, on Río del Palmar, *Pittier* 10514 (NY, US). Maricaibo, *Cleary*, Oct. 1, 1923 (NY). Bolívar: "prope Angustura" [Ciudad Bolívar], *Purdie*, May 1851 (photo of type ex K, NY negative, new series no. 2678). La Prisión, Medio Caura, *Ll. Williams* 11646 (F, US). Serrania de Guayapo, Bajo Caura, *Ll. Williams* 11832 (F, US).

COLOMBIA: (Local names, *colorado*, *palo colorado*, *balaustre*.) Atlántico: Los Pendales, *Dugand* 1126 (F, US). Tocagua, *Bro. Elias* 1580 (F, NY, US).

Bentham distinguished var. "*Orenocense*" from the typical variety by "foliolis glabrioribus basi vix cordatis." However, from the limited amount of material available, these distinctions do not seem to be sharp, and in at least two collections, *Pittier* 6611 and 10514, there is a tendency toward both types of leaflets. As more collections are made, the desirability of reducing the two varieties to synonymy may become more obvious. Maintaining the taxon *orinocense* in specific rank certainly does not seem to be justified.

The type collection of *C. patinense*, consisting of fruits and immature leaves, appears to be essentially the same as material of *C. paraense* var. *orinocense*. Most of the leaflets of *C. patinense* are rounded at the base, but a few are subcordate. The fruits (possibly submature), although slightly smaller than average, are within the size range for *C. paraense*.

# ENTOMOLOGY.—*Notes on* Pleochaetis *Jordan, 1933, from Colombia, with the description of a new species* (Siphonaptera: Ceratophyllidae).

PHYLLIS T. JOHNSON.[1] (Communicated by Robert Traub.)

There has been considerable confusion in the minds of specialists in Siphonaptera as to the status of *Pleochaetis equatoris equatoris* (Jordan, 1933) and *Pleochaetis apollinaris* (Jordan and Rothschild, 1921), due in part to our lack of knowledge of the true range of these forms. Recent collections of Siphonaptera from Colombia made by Dr. Philip Hershkovitz, Chicago Natural History Museum, have clarified the status of these two species by providing the male of *apollinaris* and the true female of *equatoris equatoris*, and have yielded a new species of the same genus as well. These are described, and a key to these forms and the remaining South American species, *Pleochaetis dolens quitanus* (Jordan, 1931) is included.

I am indebted to F. G. A. M. Smit of the British Museum (Tring) who graciously lent paratype specimens of *Pleochaetis apollinaris* (Jordan and Rothschild, 1921) and *P. equatoris equatoris* (Jordan, 1933), and who studied the holotypes of these species for me.

**Pleochaetis apollinaris** (Jordan and Rothschild, 1921)

FIGS. 5, 9, 14, 18, 19, 22, 27, 29

*P. apollinaris* was described from two females *ex Mustela affinis*, savannah of Bogotá, Colombia. At hand are the paratype female of this species, lent by Mr. Smit, and one male and three females *ex Mustela frenata*, Río Balcones, Guasco, Dept. of Cundinamarca, Colombia, summer 1952, P. Hershkovitz collector. A diagnosis of the male of *P. apollinaris* and a brief description of the species follows.

*Diagnosis.*—(male) Close to *P. equatoris equatoris* (Jordan, 1933). Separable from *P. equatoris equatoris* and *P. dolens quitanus* (Jordan, 1931) by having two ventrolateral bristles on the eighth tergum (Fig. 22), not with one bristle in this position (Fig. 24). Further separable from *e. equatoris* in that the lowest large bristle on the posterior margin of movable finger is inserted well above the level of the notch on the anterior margin of finger (Fig. 14, *F.*), not on a level with the notch (Fig. 15, *F.*); distal arm of ninth sternum with rounded proximal lobe bearing two relatively large bristles (Fig. 18), not with this lobe flattened and bearing three or four relatively large bristles (Fig. 17).

*Description.*—HEAD: Preantennal area with two rows of bristles, the first of seven medium-sized bristles, the second (ocular row) of three large bristles.

THORAX: Pronotum narrow, its dorsal margin not so long as dorsal comb spine. Pronotal comb of 19 or 20 spines.

LEGS: Protibia with six dorsal notches containing paired bristles (including apical group) (Fig. 9); meso- and metatibia with five dorsal notches proximal to the one unpaired dorsal bristle (Fig. 11).

ABDOMEN: Basal abdominal sternum of female with striations on posterior half very close together and strongly curved.

MALE: Eighth tergum (Fig. 22) with two ventrolateral bristles and four dorsolateral bristles; its caudal edge somewhat serrate. Eighth sternum (Fig. 27) with three rather heavy bristles on ventral margin subapically and several smaller bristles scattered along this margin. Distal arm of ninth sternum (Fig. 18) with proximal lobe smoothly rounded caudally and bearing a few small pale bristles plus two larger bristles. Shape of process and movable finger of clasper as in Fig. 14; the most ventral of the large bristles on *F.* inserted well above level of notch on anterior margin. Aedeagus (Fig. 19) with median dorsal lobe (*M.D.L.*) smoothly and broadly rounded dorsally, not heavily sclerotized; crochet (*CR.*) with narrowly rounded apex and concave posteroventral margin.

FEMALE: Seventh sternum (Fig. 5, A, B, and C) with narrow sinus; lobe above sinus sharply rounded, small. Eighth tergum with about eight small bristles dorsal to the spiracle. Shape and size of spermatheca body variable; striations on body not extending on to base of tail (Fig. 29, A, B, and C); tail curved over body and its apex reaching level of insertion of spermathecal duct into body.

[1] Department of Entomology, Army Medical Service Graduate School, Walter Reed Army Medical Center, Washington, D. C.

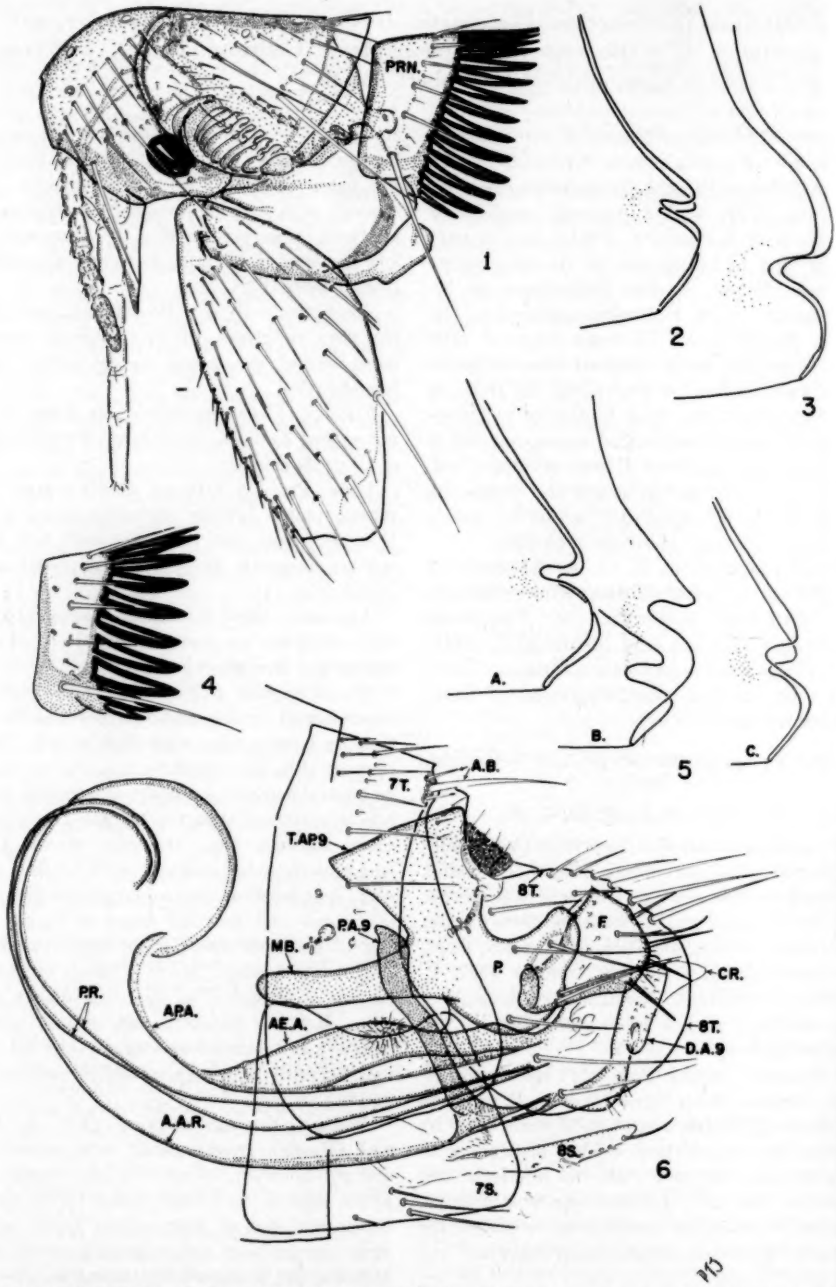



FIG. 1.—*Pleochaetis smiti*, n. sp.: Head and prothorax, holotype. FIG. 2.—*P. equatoris equatoris* (Jordan, 1933): Seventh sternum, female. FIG. 3.—*P. smiti*, n. sp.: Seventh sternum, allotype. FIG. 4.—*P. equatoris equatoris* (Jordan, 1933): pronotum, male. FIG. 5, A, B, and C.—*P. apollinaris* (Jordan and Rothschild, 1921): Seventh sternum variations, female. FIG. 6.—*P. smiti*, n. sp.: Modified segments, holotype.

*Lengths.*—Male 2.1 mm, females 2.2 mm (paratype) – 2.5 mm.

Male and one female deposited in the collections of the Chicago Natural History Museum; two females deposited in the collection of Robert Traub.

**Pleochaetis equatoris equatoris** (Jordan, 1933)

FIGS. 2, 4, 11, 15, 17, 20, 24, 28, 30

Dr. Jordan described *P. equatoris equatoris* from two specimens; the holotype male *ex Sigmodon* sp., Quebrada of Pichan, west side of Pichincha, Ecuador, and a paratype female *ex Oryzomys* sp., Paramo de Guamani, road to Baiza, region oriental, Ecuador. While studying the specimens collected by Dr. Hershkovitz and the female paratype of *equatoris equatoris*, it became apparent that the female ascribed to *P. equatoris equatoris* (Jordan, 1933) is not that species, but a representative of the new species described later in this paper. One male and one female *Pleochaetis, ex Rhipidomys* sp., San Cristobal, Bogotá, Dept. of Cundinamarca, Colombia, July 9, 1952, P. Hershkovitz collector, have proved to be *P. equatoris equatoris* (Jordan, 1933). A diagnosis of the female and short description of the species follows.

*Diagnosis.*—(female) Separable from *P. dolens quitanus* (Jordan, 1931) in that the seventh sternum possesses a sinus (Fig. 2), not lacking sinus and with truncate ventrolateral lobe. Separable from *P. apollinaris* (Jordan and Rothschild, 1921) in that the spermatheca has striations extending on to base of tail, and apex of tail ends short of level of insertion of spermathecal duct into body (Fig. 30), not lacking striations on base of tail or with a longer tail (Fig. 29); eighth tergum with only four small bristles above spiracle, not eight.

*Description.*—Head, thorax, legs, and abdomen essentially as in *P. apollinaris*.

MALE: Movable finger of clasper with lowest bristle on caudal margin on a level with the notch on anterior margin (Fig. 15, *F.*). The Colombian specimen differs somewhat from holotype *e. equatoris* in shape of the movable finger, which is more rounded dorsally than in holotype, and the apical portion of the process (*P.*) which is broader than in the holotype. Eighth tergum (Fig. 24) with one ventrolateral bristle and two dorsolateral bristles. Eighth sternum (Fig. 28) similar to *apollinaris*. Distal arm of ninth sternum (Fig. 17) with proximal lobe squared, caudal margin virtually straight, with three or four rather large bristles and several smaller ones. Median dorsal lobe of aedeagus (Fig. 20, *M.D.L.*) humped dorsally and heavily sclerotized; crochet (*CR.*) with somewhat squared apex and concave ventral margin.

FEMALE: Seventh sternum (Fig. 2) with narrow sinus and acutely rounded lobe above sinus (much as in *apollinaris*). Eighth tergum with four bristles above spiracle. Spermatheca (Fig. 30) with striations on base of tail, tail short, apically not reaching level of insertion of spermathecal duct into body.

*Lengths.*—Male 2.2 mm, female 2.7 mm.

Male and female deposited in the collections of the Chicago Natural History Museum.

**Pleochaetis smiti,** n. sp.

FIGS. 1, 3, 6–8, 10, 12, 13, 16, 21, 23, 25, 26, 31

*Type data.*—Holotype male *ex Thomasomys laniger*, Paramo, Dept. of Antioquia, Colombia, Oct. 13, 1950, P. Hershkovitz collector. Allotype female *ibid.* but Oct. 12, 1950. Paratype female *ex Oryzomys* sp., Paramo de Guamini, road to Baiza, region oriental, Ecuador, July 27, 1931, Dr. F. Spillmann collector. Holotype and allotype deposited in the collections of the Chicago Natural History Museum; paratype returned to the British Museum (Tring).

It gives me great pleasure to name this species for F. G. A. M. Smit, of the British Museum (Tring), in recognition of his important contributions to the study of Siphonaptera, and his continual helpfulness to other workers in the field.

*Diagnosis.*—Male and female separable from all other known *Pleochaetis* in possessing a pronotal comb of 23 to 26 spines (Fig. 1, *PRN.*), not with 21 spines or less (Fig. 4). Protibia with seven dorsomarginal notches containing paired bristles (Fig. 8), not five or six such notches (Fig. 9); meso- and metatibia with six dorsal pairs of bristles proximad to only single bristle on dorsal margin (Fig. 10), not with five pairs in this position (Fig. 11).

*Description.*—HEAD, MALE (Fig. 1): Preantennal area with two rows of bristles, the ocular row consisting of three long bristles plus an anterior row of nine smaller bristles (six or seven in female). Three postantennal rows arranged (on a side) 3–5–7 in male, 3(2)–5(4)–6(7, 8) in female. Apical bristles on second antennal segment extending no more than half length of club.

THORAX (Fig. 7): Pronotal comb of 26 spines (23–25 in female), the most dorsal spines no longer than dorsal margin of pronotum (Fig. 1, *PRN.*). Mesonotum (*MSN.*) with three rows of bristles plus scattered anterior bristles. Metanotum (*MTN.*) with two and one-half or three irregular rows of bristles. Lateral metanotal area (*L.M.*) with vertical row of three bristles. Metepimere (*MTM.*) with two rows of bristles plus one submarginal posterior bristle.

LEGS: Procoxa with 35 or more external bristles excluding marginals. Protibia (Fig. 8) with seven dorsomarginal notches containing paired bristles. Meso- and metatibia with six dorsal notches containing paired bristles proximad to only single dorsal bristle (Fig. 10); metatibia with 19 or 20 bristles on external surface (15 or 16 in female), not including ventro- and dorsomarginal bristles.

ABDOMEN: Basal abdominal sternum of female with close-set striations on posterior half straight,

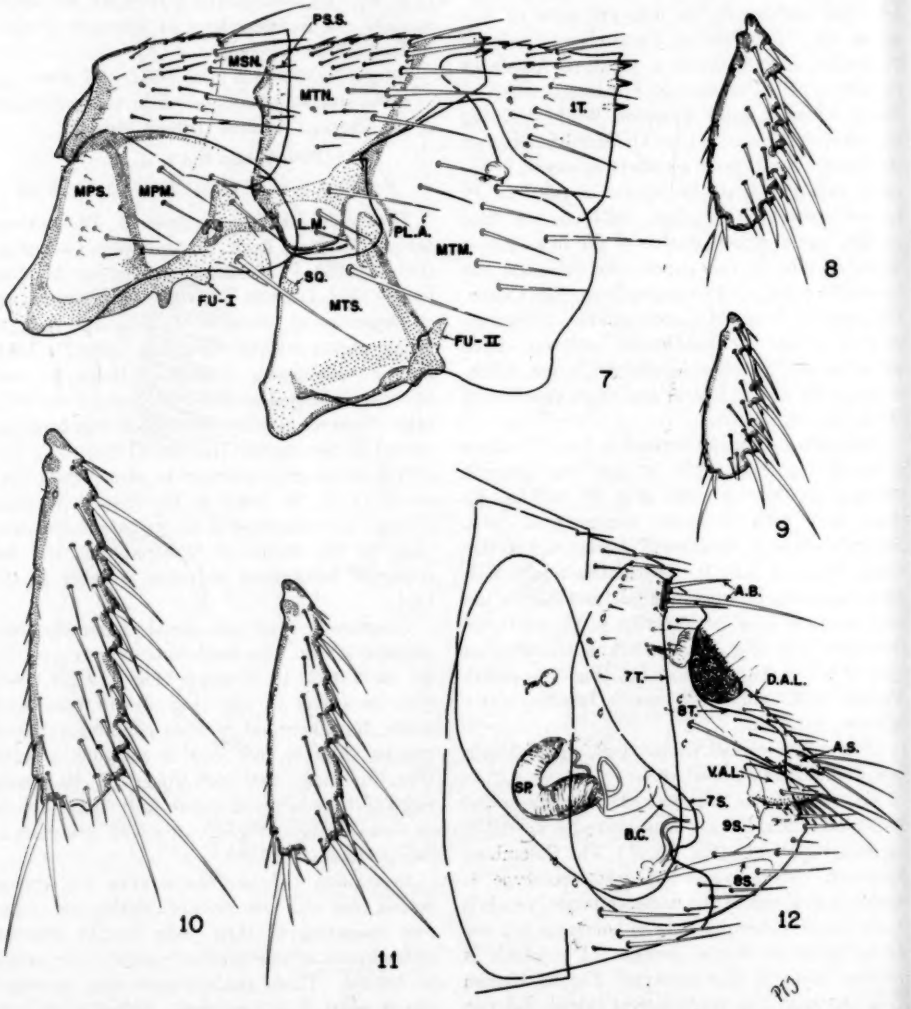



FIG. 7.—*Pleochaetis smiti*, n. sp.: Meso- and metathorax, holotype. FIG. 8.—*Ibid.*: Protibia, holotype. FIG. 9.—*P. apollinaris* (Jordan and Rothschild, 1921): Protibia, male. FIG. 10.—*P. smiti*, n. sp.: Metatibia, holotype. FIG. 11.—*P. equatoris equatoris* (Jordan, 1933): Metatibia, male. FIG. 12.—*P. smiti*, n. sp.: Modified segments, allotype.

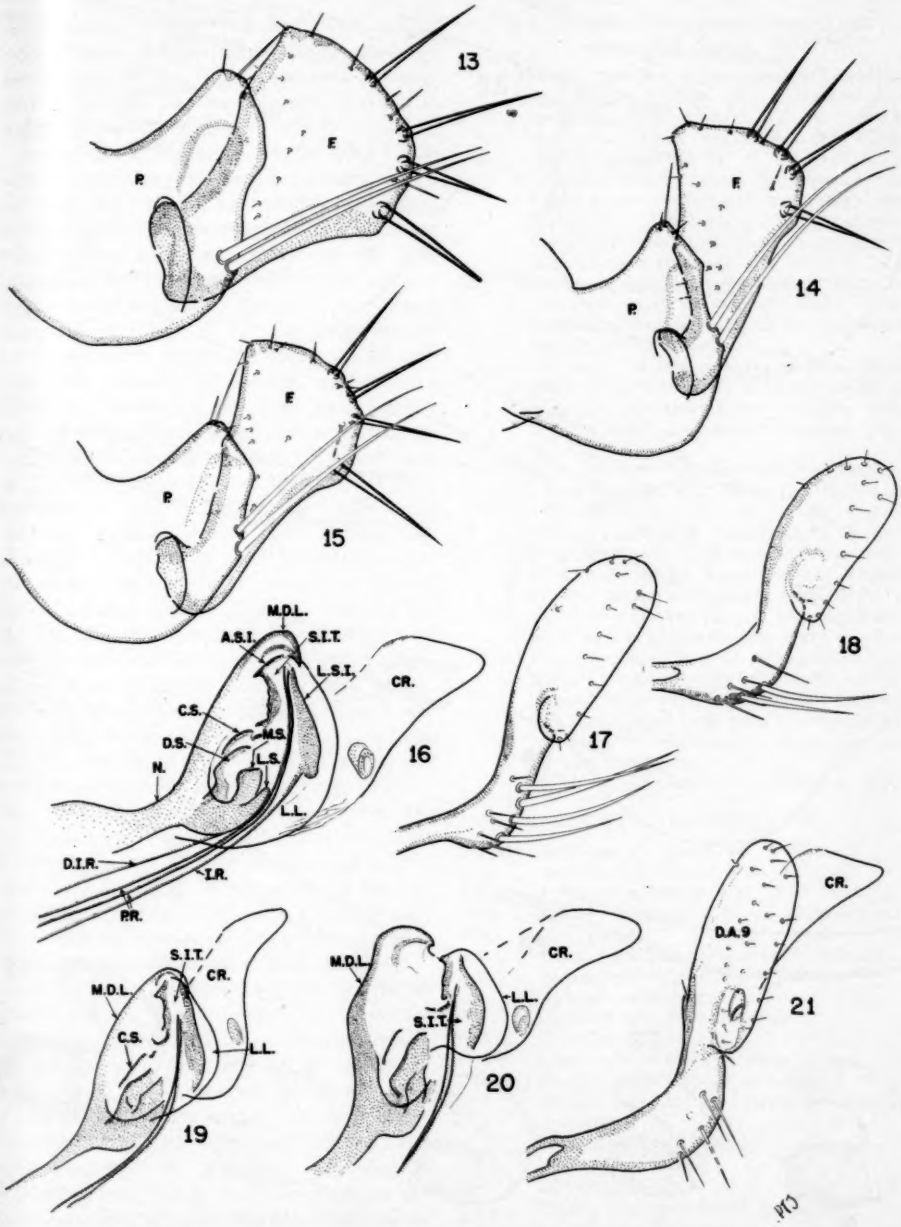



FIG. 13.—*Pleochaetis smiti*, n. sp.: Process and movable finger of clasper, male. FIG. 14.—*P. apollinaris* (Jordan and Rothschild, 1921): Process and movable finger of clasper, male. FIG. 15.—*P. equatoris equatoris* (Jordan, 1933): Process and movable finger of clasper, male. FIG. 16.—*P. smiti*, n. sp.: Apex of aedeagus, holotype. FIG. 17.—*P. equatoris equatoris* (Jordan, 1933): Distal arm of ninth sternum, male. FIG. 18.—*P. apollinaris* (Jordan and Rothschild, 1921): Distal arm of ninth sternum, male. FIG. 19.—*Ibid.*: Apex of aedeagus, male. FIG. 20.—*P. equatoris equatoris* (Jordan, 1933): Apex of aedeagus, male. FIG. 21.—*P. smiti*, n. sp.: Distal arm of ninth sternum, holotype.

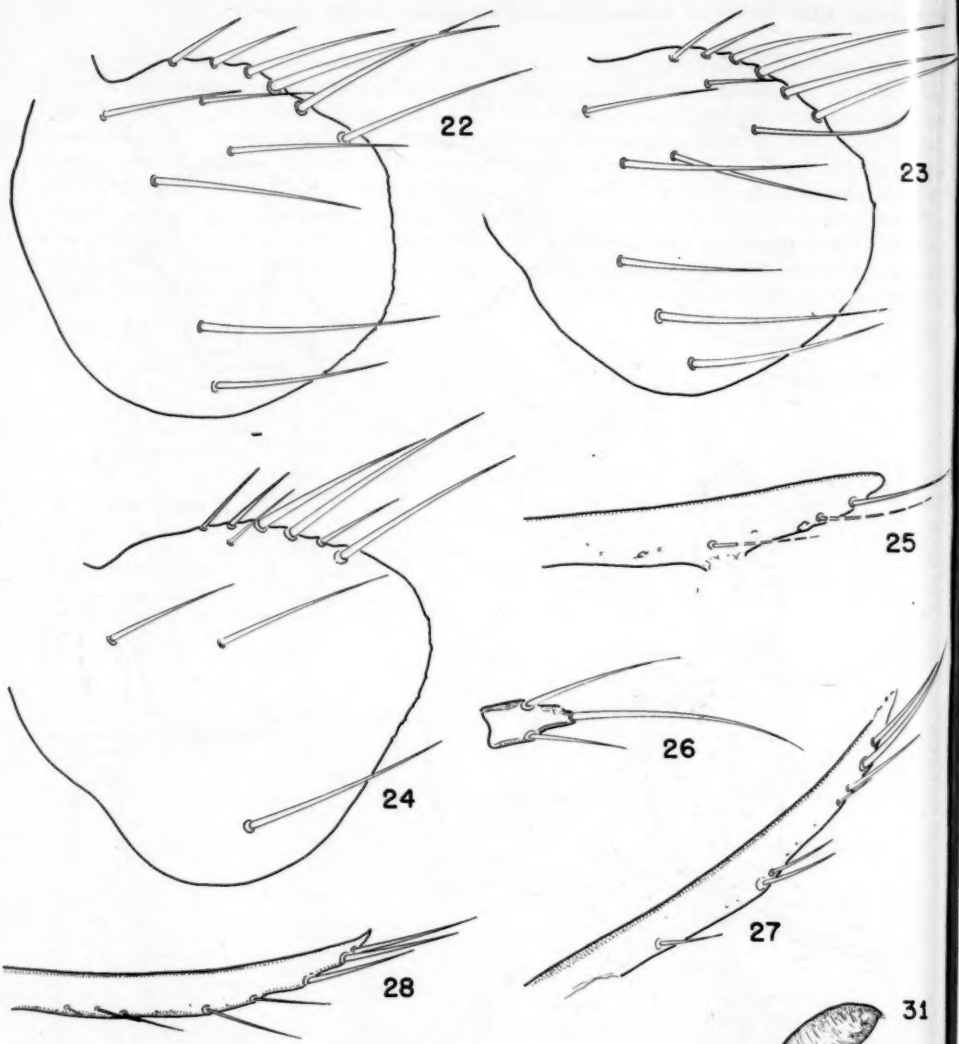



FIG. 22.—*Pleochaetis apollinaris* (Jordan and Rothschild, 1921): Eighth tergum, male. FIG. 23.—*P. smiti*, n. sp.: Eighth tergum, holotype. FIG. 24.—*P. equatoris equatoris* (Jordan, 1933): Eighth tergum, male. FIG. 25.—*P. smiti*, n. sp.: Eighth sternum, holotype. FIG. 26.—*Ibid.*: Anal stylet, allotype. FIG. 27.—*P. apollinaris* (Jordan and Rothschild, 1921): Eighth sternum, male. FIG. 28.—*P. equatoris equatoris* (Jordan, 1933): Eighth sternum, male. FIG. 29, A, B, and C.—*P. apollinaris* (Jordan and Rothschild, 1921): Spermatheca variations, female. FIG. 30.—*P. equatoris equatoris* (Jordan, 1933): Spermatheca, female. FIG. 31.—*P. smiti*, n. sp.: Spermatheca, allotype.

not markedly curved. With two well-developed rows of bristles on more anterior terga plus a few anterodorsal bristles. Apical spinelets on terga I–IV (both sides together) arranged 6–6–5–4 in male, 6(7)-6-4-2(4) in female.

MALE (Fig. 6): Eighth tergum (*8T.* and Fig. 6) with six dorsomarginal bristles, laterally, dorsal half with five bristles, ventral half with three bristles. Eighth sternum (*8S.* and Fig. 25) lacking a serrate apical appendage, broken and with most bristles missing (Fig. 25 shows estimated size and length of missing bristles). Immovable process of clasper (*P.* and Fig. 13) apically rather broad; dorsal portion of posterior margin almost perpendicular, followed by median shallow bay; acetabular bristles set on slight protuberance below this bay; the protuberance extending no farther caudad than upper "angle" above shallow bay. Movable finger (*F.* and Fig. 13) less than two times as high as broad, the lowest large bristle on posterior margin blunt apically, set somewhat below level of notch on anterior margin; margin above lowest bristle is broadly rounded. Distal arm of ninth sternum (*D.A.9* and Fig. 21) with proximal lobe broadly and evenly rounded caudally, most of bristles broken, but apparently with only relatively small bristles in this area (Fig. 21 shows estimated breadth and length of missing bristles). Aedeagal apodeme (*AE.A.*) with short neck (Fig. 6, *N.*) just anteriad to endchamber; apical appendage (*AP.A.*) long. Apex of sclerotized inner tube (Fig. 16, *A.I.T.*) as long as crescent sclerite (*C.S.*); band of inner tube not visible. Median dorsal lobe (*M.D.L.*) evenly rounded dorsally, not heavily sclerotized. Crochets (*CR.*) with posteroventral margin slightly concave; dorsal margin straight, anterodorsal angle rounded, somewhat rugose.

FEMALE (Fig. 12): Posterior margin of seventh sternum (*7S.* and Fig. 3) bilobed, the lobes subequal, sinus between lobes rounded, broadly triangulate, much broader than in *apollinaris* and *equatoris*. Eighth tergum (*8T.*) with seven or eight small bristles above spiracle. Spermatheca (*SP.* and Fig. 31) with juncture of head and tail not marked, tail gradually narrowing to subrounded apex which does not extend as far as apex of body; tail with striations visible along entire length. Anal stylet (*A.S.* and Fig. 26) about two times as long as broad.

*Lengths.*—Holotype 2.8 mm, allotype 2.9 mm, paratype 3.2 mm.

KEY TO THE SOUTH AMERICAN SPECIES OF PLEOCHAETIS JORDAN, 1933

1. Protibia with seven dorsal notches containing paired bristles (Fig. 7); meso- and metatibia with six dorsal notches containing paired bristles proximal to only single dorsal bristle (Fig. 10) ..... *smiti*, n. sp.
   Protibia with five or six dorsal notches containing paired bristles (Fig. 9); meso- and metatibia with five dorsal notches containing paired bristles proximal to only single dorsal bristle (Fig. 11) ..... 2
2.(1) Female seventh sternum lacking sinus in posterior margin, ventrolaterally truncate; movable finger of male with posterior margin evenly rounded
   *dolens quitanus* (Jordan, 1931)
   Female seventh sternum with narrow sinus (Fig. 5); movable finger of male with definite angle on posterior margin (Fig. 15, *F.*) ..... 3
3.(2) Eighth tergum of female with four bristles above spiracle, base of spermatheca tail with striations (Fig. 30); male eighth tergum with one ventrolateral bristle (Fig. 24); lowest large bristle on posterior margin of movable finger inserted at level of notch on anterior margin (Fig. 15, *F.*)
   *equatoris equatoris* (Jordan, 1933)
   Eighth tergum of female with eight bristles above spiracle, base of spermatheca tail lacking striations (Fig. 29); male eighth tergum with two ventrolateral bristles (Fig. 22), lowest large bristle on posterior margin of *F*, inserted well above level of anterior notch (Fig. 14, *F.*)
   *apollinaris* (Jordan and Rothschild, 1921)

LIST OF ABBREVIATIONS

| | |
|---|---|
| A.A.R. | Aedeagal apodemal rod. |
| A.B. | Antesensilial bristle. |
| AE.A. | Aedeagal apodeme. |
| AP.A. | Apical appendage of aedeagal apodeme. |
| A.S. | Anal stylet. |
| A.S.I. | Apex of sclerotized inner tube, aedeagus. |
| B.C. | Bursa copulatrix. |
| CR. | Crochet, aedeagus. |
| C.S. | Crescent sclerite, aedeagus. |
| D.A.L. | Dorsal anal lobe. |
| D.A.9 | Distal arm of ninth sternum. |
| D.I.R. | Dorsal intramural rod, aedeagus. |
| D.S. | Dorsal sclerite of apodemal strut, aedeagus |
| F. | Movable finger of clasper. |
| FU.I | Mesosternal furca. |
| FU.-II | Metasternal furca. |
| I.R. | Ventral intramural rod, aedeagus. |
| L.L. | Lateral lobe, aedeagus. |
| L.M. | Lateral metanotal area. |
| L.S.I. | Lateral sclerite of sclerotized inner tube, aedeagus. |
| MB. | Manubrium of clasper. |

M.D.L. Median dorsal lobe, aedeagus.
MPM. Mesepimere.
MPS. Mesepisternum.
M.S. Median sclerite of apodemal strut, aedeagus.
MSN. Mesonotum.
MTM. Metepimere.
MTN. Metanotum.
MTS. Metepisternum.
N. Neck, aedeagus.
P. Immovable process of clasper.
P.A.9 Proximal arm of ninth sternum.
PL.A. Pleural arch.
P.R. Penis rods, aedeagus.
PRN. Pronotum.
PS.S. Pseudosetae.
S.I.T. Sclerotized inner tube, aedeagus.
SP. Spermatheca.
SQ. Squamulum.
T.AP.9 Tergal apodeme of segment nine.
V.A.L. Ventral anal lobe.
1T. First tergum.
7S. Seventh sternum.
7T. Seventh tergum.
8S. Eighth sternum.
8T. Eighth tergum.
9S. Ninth sternum.

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## ZOOLOGY.—*New names for two genera of Octocorallia.* FREDERICK M. BAYER, U. S. National Museum.

During preparation of the chapter on Octocorallia for the Treatise on Invertebrate Paleontology, R. C. Moore, editor, it has come to my attention that certain generic names in the Coelenterata Octocorallia must fall as homonyms of accepted generic names in other animal groups. Two of these are the following:

(1) *Fascicularia* Viguier, 1888, page 186 [type, *Fascicularia radicans* Viguier = *Paralcyonium edwardsii* deLacaze-Duthiers]; *nec* Dybowski, 1873 (Tetracorallia); *non* Lyell, 1839 (Bryozoa); *non* Lamarck, 1816 (Hexacorallia). For this thrice preoccupied generic name I propose to substitute **Viguieriotes,** new name [type, *Paralcyonium edwardsii* de Lacaze-Duthiers, 1888]. The family based upon this genus thus will henceforth be called **Viguieriotidae.**

(2) *Stenella* J. E. Gray, 1870, page 48 [type, *Primnoa imbricata* J. Y. Johnson, 1862]; *nec* J. E. Gray, 1866 [Cetacea]. As a replacement for this preoccupied name I propose **Candidella,** new name [type, *Primnoa imbricata* J. Y. Johnson, 1862].

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## *NOTICE*

The Federation of American Scientists is making a new survey of the effects on science of the visa policies in existence under the present United States immigration act. They are interested in information about: (1) Individuals who wished to visit the United States for professional reasons but who have been prevented from coming because of visa refusal or delay. (2) Individuals who received invitations to come to this country on professional business (e.g., to attend meetings, as a visiting lecturer, etc.) but who refrained from applying for a visitor's visa because of present visa policies. (3) Visitors who experienced visa difficulties after reaching the United States. (4) Scientific congresses, meetings, or symposia that were not scheduled in the United States because of current visa policies.

The Federation would like information about such cases that have occurred in the past two years. It would be helpful if as many details as possible were given. In each specific case, the Federation would like to know to what extent this information must be treated as confidential. Information should be sent to the Federation of American Scientists Committee on Visa Problems, P. O. Box 1191, Stanford, Calif.